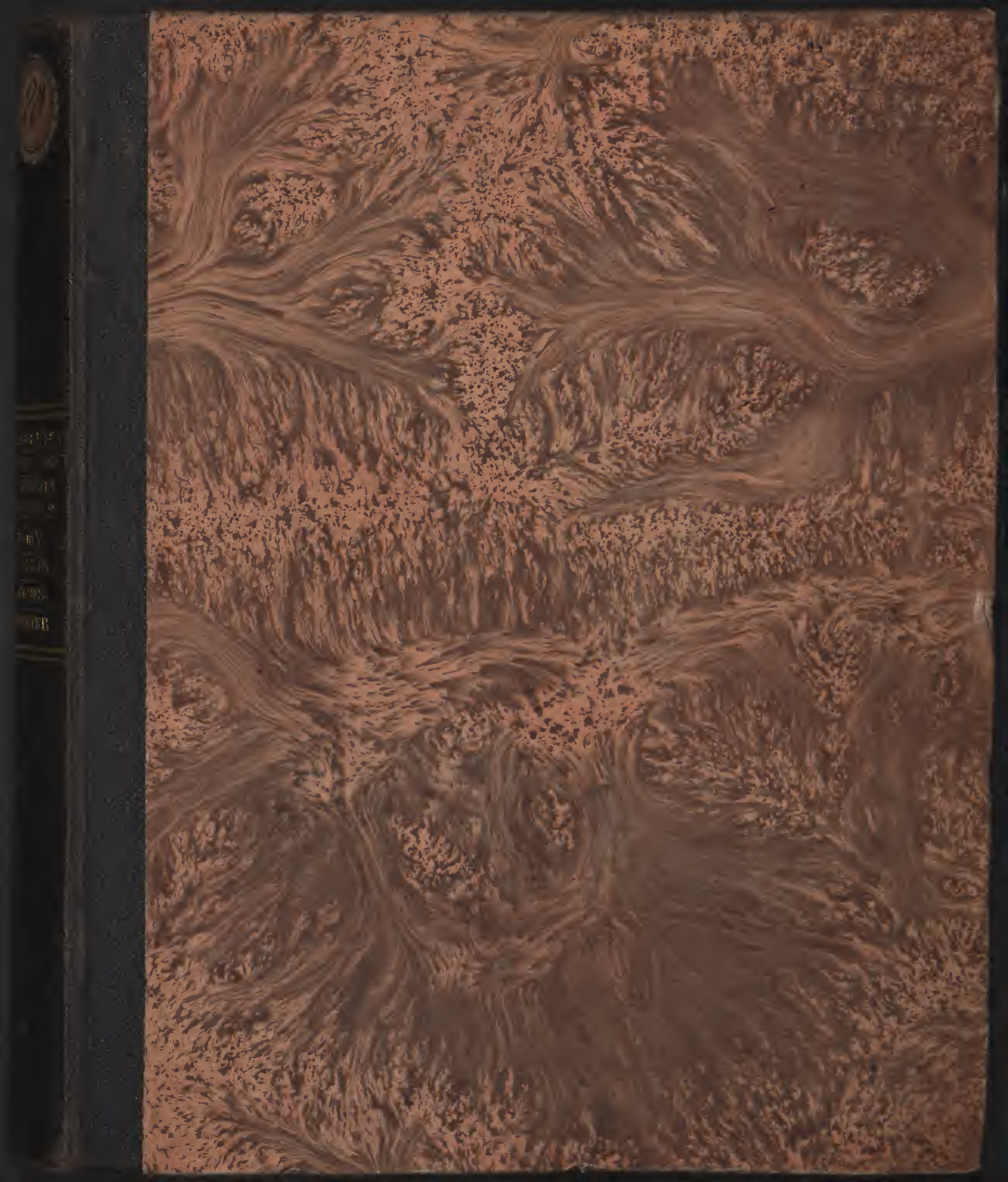

REALE ACCADEMIA DELLE SCIENZE DI TORINO

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SUPPLEMENT

TO

DR. BRADLEY'S MISCELLANEOUS WORKS:

WITH AN ACCOUNT OF

HARRIOT'S ASTRONOMICAL PAPERS.

ACCADEMIA
R. DELLE SCIENZE
DI TORINO

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ADVERTISEMENT.

In the Appendix to Bradley's Miscellaneous Works (p. 522) I have ventured to say that no dependence was to be placed on the common account of Harriot's papers. That account, however, was given to the world by one, whose name has now been associated with the cause of astronomy for nearly half a century; it has in some quarters been very readily admitted, and unfounded accusations have been repeatedly derived from it against those, by whose fault it was supposed that the papers were withheld from the public. It was necessary therefore not to leave the contradiction to rest on my bare assertion; and as the discussion had begun in connexion with the publication of Dr. Bradley's works, this supplement appeared to offer no inappropriate opportunity for a full statement of the truth. I have to regret that the scientific world has been deprived of the Baron de Zach before it was possible for me to complete the inquiry: but this has made me doubly careful not to overstate any thing. I have endeavoured also to keep to facts, on which the reader will be well able to form his own opinion, and on the accuracy of which he may, in many parts, easily satisfy himself without requiring any assistance or further explanation. If I have been led on to a greater length than at first sight may appear necessary, it must be recollected that much more detail is requisite to efface a false impression than to satisfy unprejudiced minds. Upon the whole, I trust that I have proved the erroneous and exaggerated view which has been taken of the subject; that the papers sent by the Baron de Zach in 1794 to Oxford did not supply materials for a new edition of Harriot's works, which, it must be remembered, was the original and real design; and, that if such a publication had been unwisely attempted with this imperfect selection, much that was really interesting, and which had never been communicated, must of necessity have been left entirely unnoticed.

S. P. RIGAUD.

March 1833.

Analysis of Calculations made by Bradley when investigating a Rule for Refraction.

WHEN engaged in the publication of Dr. Bradley's Miscellaneous Works, I had the good fortune to meet at Greenwich with a paper book, in which he was in the habit of writing occasional memoranda, and of making the calculations on which he was employed. It is the same to which Dr. Maskelyne has referred in his Dissertation on the Latitude of Greenwich [a]: it furnished me with several curious notices for the Memoirs of their author [b], but it also contained much more, of which I was not able at the same time to avail myself. Dr. Maskelyne, although he first published Bradley's rule for refraction, has given us no particulars of the specific manner in which it was investigated. Now the Greenwich book contained a large number of computations made for that purpose. This was clear from sentences, which were occasionally inserted in them; but the quantities which were used, had for the most part no descriptions annexed to them to indicate the objects, for which they were introduced; natural numbers were mixed with logarithms, the logarithms were most usually without indices, and (what at first created considerable difficulty) in some cases the five first figures were used, and in others the five last of the seven decimals, which are given in common tables. These obstacles have probably contributed to deter others from examining the book, although it has now remained for so many years in the library of the Royal Observatory. I thought it best not to stop the publication of the other papers by dwelling at the time on an examination, the result of which was attended with so much uncertainty; but being allowed to retain the manuscript till I was able to apply more undivided attention to its contents, I gradually became better acquainted with its several parts. In this I had an advantage, from being able to refer to Bradley's book at Oxford, in which he entered his "Observed Refractions," and, although we might wish for more detailed information on some points connected with so interesting a subject, it is hoped that the following particulars, combined with what has been already given in the Memoirs [c], will enable the reader to form a fair idea of the manner, in which Bradley conducted his inquiry.

Descriptions must be tedious, and could not be easily made intelligible; a facsimile is therefore given (in the upper part of plate I. Sup.) of the first numbers which occur in Bradley's calculations. He began, as Dr. Maskelyne has explained [d], by determining the apparent zenith distances of the equator and of the pole, which he found together to be 89° 58′ 6″. This was deficient of 90°, by 1′ 54″, which quantity being the sum of the refractions for these two altitudes, he divided in the ratio of the tangents of the respective zenith distances. His method of calculation for this purpose is too obvious

[a] Phil. Trans. vol. lxxvii. p. 156, 160.

[b] P. xxiv. lxxxvi. lxxxvii. xc.

[c] P. lxxxv—xc.

[d] Bradley's Misc. Works, P. 75.

to call for any thing like an explanation of it; but some little time and trouble may be saved by an enumeration of the quantities to which his numbers refer; and for that purpose the following particulars may not be wholly useless.

He took tang. Z. D. of equator + tang. Z. D. of pole : tang. Z. D. of pole :: sum of the refractions : refraction for the Z. D. of the pole;

for 125530 is the nat. tang. of 51° 27′ 30″
79575 38 30 40
205105

9.900778 = log. tang. 38° 30′ 40″
−1.311976 = log. 20.5105 the sum of the tangents.
2.060698 = log. 115″
1.649500 = log. 44″.6

10.098747 (the tangent of 51° 27′ 30″) was originally 9.893293, (the sine of the same quantity which was possibly taken out by mistake,) but is only used to find the natural tang. From the sum of the refraction he separately calculated the difference of them, and the half of the difference would be 12″.8, if the sum is taken as 114″; and 12″.9, if it is 115″. Bradley appears at first to have written 12½, and then to have altered it to 13″, which was nearer to the truth and avoided the fraction of seconds. By this means he got his latitude sufficiently near for his first approximation, and then proceeded to investigate more particularly the effects of refraction.

The refractions, with which he began his calculations, were deduced from the observations of γ Persei for 75° 57′ 30″, γ Draconis for 76° 55′ 20″, and Capella for 82° 41′ 50″. His method of proceeding will be evident from an examination of some of his numbers in the plate; for ease of reference letters have been annexed to each row of figures, and the numbers, which will be used between brackets are intended to mark the distance from the left hand, at which the small square may be in which the quantities under consideration are situated.

The quantities in A are repeated; and it will be better therefore to begin with those in the succeeding line.

B (1) 9.9992080
+9.9964628 sine 82° 41′ 50″
9.9956708 sine 81 55 24
46′ 26″ = 82° 41′ 50″ − 81 55 26

B (2) 7.2983593 tang. 0 6 50
−8.1305824 tang. 0 46 26
9.1677769
+7.1612464 tang. 0 4 59
6.3290233 tang. 0 0 44

B (3) 9.9992080
+9.7942554 sine 38 30 40
9.7934634 sine 38 25 41
4′. 59″ = 38° 30′ 40″ − 38 25 41

B (4) 9.9992080
+9.8932930 sine 51° 27′ 30″
9.8925010 sine 51 19 39
7° 51′ = 51° 27′ 30″ − 51 19 39

B (5) 9.1677769
+7.3585965 tang. 0 7 51
6.5263734 tang. 0 1 $9\frac{1}{3}$

These numbers are evidently derived from the method of calculation described by Thomas Simpson in his Dissertation on Refraction. At p. 59 he gives the following rule: "As 1 to .9986, &c. or as radius to the sine of 86° 58′½ so is the sine of any given "zenith distance to the sine of an arc, $\frac{2}{11}$ of the difference of which arc and the zenith "distance is the refraction sought." Now the logarithm of 0.99861 is 9.9993959, which clearly answers to Bradley's repeated logarithm 9.9992080. Why he altered it to this particular quantity cannot be determined; nor would the inquiry be of much importance, for it will be found that his whole method was tentative.

Simpson assumes an horizontal refraction, and from it calculates the quantities for different altitudes by a formula which he had determined from theory. Now Bradley knew the great uncertainty of all refractions deduced from observations near the horizon; and the difficulty of reconciling them with Simpson's coefficients evidently determined the first object to which he turned his attention. He therefore took a quantity, which (no doubt for some good reason) he had a little altered from Simpson's, and with it found an auxiliary angle (46′ 26″ in B (1)). Simpson gives a constant multiplier for his angle which is to determine the refraction; but Bradley, taking not the angles themselves, but their tangents, tries, by a mutual comparison of them, to discover what this constant multiplier ought to be. For this purpose he subtracts (in B (2)) the tangent of his auxiliary angle from that of the observed refraction at the same zenith distance, and by that means gets the logarithm 9.16782. As a test of its accuracy, he adds this quantity to the tangent of the auxiliary angle, which he found (B (3)) for the zenith distance of the pole, and the sum gives the tangent of 44″, which was the observed refraction at that elevation. He repeats the same (B (4) and B (5)) for the height of the equator, and again brings out a quantity which is not exactly, but very nearly the same as that which had been deduced for it from observation[e].

This first trial seems tolerably successful; but he was induced to examine the effects of varying the quantity, by which he multiplied the sines of the zenith distances, and at last increased its log. in D (8) from 9.9992080 to 9.9999200. These alterations are evidently arbitrary; and as the same process is repeated through nearly the whole of the remaining part of the calculation, there is only one additional particular which it can be useful to explain. From some other observation the refraction for Capella S. P. appears

[e] By some oversight the height of the equator is written in the little table 51° 27′ 30″, instead of 51° 28′ 30″; but this makes no difference in the auxiliary angle or in the refraction.

to have come out 7′ 10″ instead of 6′ 50″, and the number 3190, which occurs in C (6) and afterwards, was introduced to try the consequence of it, 7.3190440 being the tang. of 0° 7′ 10.″

The numbers at the end of the line E give the refraction from observation. On the fifth of June 1751 it may be seen in the printed observations that Bradley found the zenith distance of Capella S. P. to be 82° 41′ 52″. In the book of "Observed "Refractions," there is a memorandum that, for the error of collimation, 2″ were to be subtracted from zenith distances observed at this time to the north, which will give the quantity here used. To the south, the following observations occur of the same star:

1751 June 4.	5° 45′ 57″½	5° 45′ 53″
5.	5 45 57½	5 45 54
6.	5 45 58	5 45 54

The first are from the divisions of 96 reduced to degrees and minutes; and as this was the more to be depended on, it is probable that it was used; and then 82° 41′ 50″−5° 45′ 58″=76° 55′ 52″. Again, the apparent zenith distance of the pole was 38° 30′ 38″, and twice this quantity is 77° 1′ 16″; 5′ 24″, which is the excess of 77° 1′ 16″ above 76° 55′ 52″, leaves a portion which is to be accounted for by refractions. Now, if to this difference of 5′ 24″ be added the refraction at Z. D. 5° 45′ 58″ + twice the refraction at the altitude of the pole, the sum will be equal to the refraction at Z. D. 82° 41′ 50″. Taking therefore 44″ for the polar refraction, and calculating the other from it (as Bradley most probably in this instance would have done) by the simple ratio of the tangents of zenith distances, we shall have 44″ × 2 + 5″.6 = 93″.6. This is larger than the number (1′ 26″) actually added; but then it is noted in the memorandum with respect to collimation, that 6″ were to be subtracted on the south for that and the error of the arc of the old quadrant: if this correction had been applied to 5° 45′ 58″, the difference would have been 5′ 18″ instead of 5′ 24″. Now 5′ 18″ + 1′ 33″.6 = 6′ 51″.6, which is sufficiently near to 6′ 50″ to make it probable that Bradley introduced the correction for his instrument in this sum of 1′ 26″, which he has used in the calculation.

As the opportunity occurred from observations of circumpolar stars, Bradley continued to apply the same method to them. In the first instance he had varied his logarithm through rather a wide range; but this appears to have been more to try its effect than for any other purpose. He generally began with 9.9990000, a little more or a little less. With this he found an auxiliary angle from one of his lowest stars, and by a comparison of its tangent with that of the observed refraction, arrived at a multiplier, which he tried by its results when applied to his other contemporaneous observations. According as these came out nearer or further from the truth, he varied the quantity which he had applied to the sines of the zenith distances; and with reference to this process we find the following memorandum: "N.B. When the lesser refraction "comes out too small, the log. of the ratio must be diminished; and the contrary, when "it comes out too big."

The repetition of arithmetical computations soon becomes tedious, and he therefore naturally endeavoured to diminish his labour. A logarithm which began with 9.999 would not alter the sine to which it was to be added, either in the index or in the two first places of decimals; he contented himself therefore with writing down the five last decimals, well knowing what he must prefix to the result of them. This could occasion no difficulty to him, and he was only working for himself; but the omission would not obviously occur to others, and it therefore may save trouble to point it out particularly. Thus in the book of "Observed Refractions" there are repeated entries like "91535$=z$, .1507$=x$." This means, that for the observations, to which this memorandum was annexed, 9.9991535 was found to be the best number for determining the auxiliary angles, and 9.1507 was the logarithm which, when added to the tangents of these angles, brought out the refraction so as to answer best, upon the whole, to those which resulted from immediate observation.

In this manner Bradley continued to work till he probably felt the wish of having some more direct method of finding the quantities of refraction. He knew that the simple ratio of the tangents was insufficient for large zenith distances; and the first means, by which he tried to introduce a distinct correction may be traced in the following numbers:

" 92860	25100	92860	25100	92860	25100
" 94850	21517	15494	51517	68358	81208
" 87710	46617	08354	76617	61218	06308
" 54 21½	D	63 14 54½	E	75 35 32	F
" 5 38½	2.9253	11 15½	5.8367	22 18	11.5633

" $a+b+c=\mathrm{D}$ then $a=\frac{8}{3}\mathrm{D}-\mathrm{E}+\frac{1}{12}\mathrm{F}=2.9277$ log.$=6.46653$

" $2a+4b+8c=\mathrm{E}$ $b=-2\mathrm{D}+\frac{5}{4}\mathrm{E}-\frac{1}{8}\mathrm{F}=0.0001$ log.$=2.0000$

" $4a+16b+64c=\mathrm{F}$ $c=\frac{1}{3}\mathrm{D}-\frac{1}{4}\mathrm{E}+\frac{1}{24}\mathrm{F}=-0.0023$ log.$=3.30173$."

The tangent of 45° being 1, the tangent 63° 26′ 5″.8 will be 2, and that of 75° 57′ 49″.5 will be 4. Now for the observations of the 18th of November 1750 (the examination of which came immediately before the preceding extract) Bradley had found 9.9992860 to be the best constant for finding the auxiliary angles, and 9.25100 the quantity which was to be applied to their tangents. He therefore uses these numbers to calculate the refraction for the zenith distances 45°, 63° 26′ 10″, 75° 57′ 50″. Accordingly he finds for 45°

9.25100
7.21517 tang. 5′ 38″½
6.46617 log. 0.00029253 (tang. 1′ 0″.3)

In the same manner for 63° 26′ 10″

6.76617 log. 0.00058367 (tang. 2′ 0″.4)

And for 75°.57′.50″.

7.06308 log. 0.0011563 (tang. 3′ 58″.5.)

The first and second do not differ so much from the ratio of 1 to 2 as the second and third. This must be so from the nature of the case; and Bradley therefore was induced to try whether the second and third powers of the tangents of zenith distances did not enter, as well as the first, into the required formula. To determine this, he assumed the expression that the tang. of refraction $= a.$ tang. Z. D. $+ b.$ tang.^{2} Z. D. $+ c.$ tang.^{3} Z. D. He then took the three cases in which 1st tang. Z. D. $=1$ and the expression $=$ D; 2nd tang. Z. D. $=2$, and the expression $=$ E; and 3rd tang. Z. D. $=4$, and the expression $=$ F. From these three equations he brought out a rule, thus:

$$\text{tang. of refraction} = 0.00029277.\ \text{tang. Z. D.} + 0.00000001.\ \text{tang.}^2\ \text{Z. D.} - 0.00000023.\ \text{tang.}^3\ \text{Z. D.}$$ [f]

In this result the coefficient of tang.^{2} Z. D. came out so very small, that he was induced to neglect it altogether, and by that means his formula became $a.$ tang. Z. D. $-c.$ tang.^{3} Z. D. The subject having occupied some of the first mathematicians of the present time, it is very remarkable that their investigations lead to a series depending on the odd powers of the tangents of zenith distance, and M. Laplace [g] shews that his term, involving tang.^{5} is almost insensible even at 88° dec. or 79° 12′ sexag.

Bradley examined his coefficients by trial on observed refractions, and found that they were not sufficiently accurate. This may naturally have been expected. The tangents for large arcs increase so rapidly, that no dependence could be placed on the use of their cubes, unless the quantity assumed for a multiplier had been determined with greater precision than the first inquiry could attain to. By further modification, however, he satisfied himself better on this point, and the result of his inquiry may be seen from the following memorandum:

> "Suppose the constant number to be added to the thermometer to be 450, then add log. 2.9853 to the log. of the height of the barometer, and from the sum subtract the log. of 450 increased by the degrees of the thermometer: the remaining log. will be the log. of the refraction at 45°, (in seconds,) and if to this log. be added the log. tang. of any other Z. Dist. there will be given the log. of the refraction (in seconds): this will be a little too big below 70°; but if to the log. last found be added the constant log. 6.8352, and twice the log. tang. of the Z. Dist. the sum will be the log. of seconds to be subtracted from the refraction before found. This rule will give the true refraction within two seconds, as far as 85° from the zenith."

2,9853 and 6,8352 are the logarithms of 966,72 and 0.00068423; the substance therefore of the rule may be more distinctly understood when expressed in the following algebraical formula:

$$\text{Refraction} = \frac{b}{450+t}.966''.72\ \text{tang. Z. D.}\ (1 - 0.00068423\ \text{tang.}^2\ \text{Z. D.})$$

$$\text{or}\ \frac{b}{450+t}.\ (966''.72\ \text{tang. Z. D.} - 0''.66145\ \text{tang.}^3\ \text{Z. D.})$$

If in this expression we take, according to Bradley's standard, $b=29.6$, and $t=50$

$$\text{Refraction} = 57''.23\ \text{tang. Z. D} - 0''.03916\ \text{tang.}^3\ \text{Z. D.}$$

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[f] This at first sight may seem to differ from Bradley's numbers; but as the relative results would be the same, he omitted all the noughts which strictly belong to the beginning of his values of the tangents of refraction. This was only to save unnecessary trouble in working out his equations. The indices of the logarithms which he has annexed to *a*, *b*, *c*, will shew that for making use of his formula he gave the real value to his quantities.

[g] Mec. Cel. tom. IV. p. 269.

M. Delambre[h] brought out the analogous quantity with Bradley's coefficients,

$$=56''.65 \text{ tang. Z.D} - 0''.04665 \text{ tang.}^3 \text{ Z.D.} + 0''.0000778 \text{ tang.}^5 \text{ Z.D.} - \&\text{c.}$$

It may, however, be reasonably doubted whether Bradley, who was always willing to avoid long decimals, would have taken his mean refraction at 45° to the hundredth parts of seconds; but to avoid this, we must suppose the standard height either of the barometer or of the thermometer to be altered. After a number of trials, therefore, for this purpose, it was found that log. $58'' \times \frac{500}{30}$ is 2.9852767. No other numbers brought out so close a correspondence without introducing fractions; and if these are applied to the coefficient of tang.^{3}, the rule will become

$$\text{Refraction} = \frac{b}{30} \cdot \frac{500}{450+t} \, (58'' \text{ tang. Z.D} - 0''.03967 \text{ tang.}^3 \text{ Z.D.})$$

or (without involving the corrections for the barometer and thermometer)

$$\text{Refraction} = 58'' \text{ tang. Z.D.} - 0''.03967 \text{ tang.}^3 \text{ Z.D.}$$

There are two examples of the application of this rule: one is for the refraction at Z. D. 85°, and is most probably compared with the refraction of Nov. 17, 1750, with which it seems to agree: the other is for the zenith distance of 82° 42′, which Capella was observed to have on the 10th of July, 1750; and as the heights of the barometer and thermometer answer to what they were at that time, there can be little doubt of the star referred to. In this case the result of the computation exceeds that from observation by 1″.3. Bradley however was not yet satisfied without endeavouring to arrive at greater accuracy by a further modification.

In the beginning of the very next page we find

> "To the log. A=$\overset{98965}{2.98966}$, add the log. of b=the height of the barometer, and the log. tangent of the given zenith distance, and from the sum subtract the log. of $450+t$, (t being the degrees of the thermometer in Fahrenheit's scale) then the difference will be the log. of the refraction required, if the zenith distance be less than 60°.
>
> "But if the zenith distance be greater, then to the given log. C=$\overset{7.21527}{7.21534}$ add log. of the height of the barometer, and three times the logarithmic tang. of the given zenith distance. The sum will be the log. of the seconds to be subtracted from the refraction before found. This last part is supposed to be proportional to the elasticity of the air, (or the height of the barometer,) and the first part is supposed proportional to the density of the air."

2.98966 and 7.21534 are the logarithms of 976.47 and 0.001642; the rule, therefore, if reduced to a formula, becomes

$$\text{Refraction} = \frac{b}{450+t} \, 976''.47 \text{ tang. Z.D.} - 0''.001642 \, b. \text{ tang.}^3 \text{ Z.D.}$$

If the standard quantities are again taken as 29.6 and 50, the expression becomes

$$\frac{b}{29.6} \cdot \frac{500}{450+t} \cdot 57''.806 \text{ tang. Z.D.} - \frac{b}{29.6} \cdot 0.04860 \text{ tang.}^3 \text{ Z.D.}$$

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[h] Astron. vol. I. p. 303.

but here again is the objection from decimals, and as the constant logarithm 2.98966 only exceeds 2.9853, which was adopted in the previous rule, by 0.00436, it is probable that a further modification of some of the elements has been introduced. There is written immediately under it "$45^{\circ}=61.0''$," but this probably refers to the example rather than to the rule. After varying the quantities in every way, which he was likely to have used, the nearest expression was found from $58''\frac{500}{29.7}$, the logarithm of which is 2.9896416.

These quantities, brought out without any previous view to such a result, happen to have an analogy, which tends much to confirm the probability of their really containing Bradley's numbers; for the first $\left(58.\frac{500}{30}\right)$ only differs from the second $\left(58\frac{500}{29.7}\right)$ by an excess of 0.3 in the standard of the barometer. Assuming this to be the case, the rule will become

$$\text{Refraction}=\frac{b}{29.7}\times\frac{500}{450+t}58''\text{ tang. Z. D.}-\frac{b}{29.7}0''.048756\text{ tang.}^3\text{ Z. D.}$$

In this form there is not only an alteration of the coefficients, but there is an essential variation in another respect. It has been found that there is a part of the refraction, which requires correction for the state of the barometer, while it is quite independent of the thermometer. Bradley has contemplated this very distinction. He has not indeed connected it with the proper function of the zenith distance, $\left(\text{which is }\frac{\text{tang.}}{\text{cos.}^2}\right)$ nor does his coefficient agree with that (0.0739) which Dr. Brinkley[i] arrived at from later and more recondite investigations; but in the earlier stage of the inquiry there are not many, to whom *à priori* the possibility of such a distinction could have occurred.

The next step was to the form in which Bradley finally embodied his rule[k].

"If to the log. 7.5686 be added the log. of the height of the baronı. and log. tang. of the apparent zenith distance lessened by three times the refraction[l], (which is given near enough in the common tables of refraction,) and from that sum be subtracted the logarithm of 348, increased by the degree of the thermometer, the residue will be the log. of the correct refraction in minutes, &c. &c."

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[i] Trans. of the Royal Irish Acadamy, vol. XII. p. 99.

[k] On a loose paper at Oxford there is the following memorandum:

$y=393$ Refraction in seconds
Log. $n=8.9820$ $=\text{log. }m.+\text{log. }b.+\text{log. }t.-\text{log. }(393+a)$
Lon. $m=2.9311$
$b=$barometer subtract. log. $n.\ t^{8}$
$a=$thermometer
$t=$tang. of App. Z. Dist.

It is unnecessary to enter into an analysis of this formula, of which Bradley does not appear to have made any use: it only shews that he was willing to try whether $\frac{8}{3}$ or a somewhat lower power than 3 of his tangent might not give him a more accurate correction.

[l] It is on a vacant space, which appears to have been left over the rule, that we find the remark (quoted, Memoirs, P. lxxxvii.) of the subtracted arc being possibly "in summer $3\frac{1}{2}$, "in winter $2\frac{1}{2}$," and not always three times the refraction. We find, probably upon the same ground, that he at first separated his "morning" from his "evening refractions."

There is a little omission in wording the latter part of the rule, which might easily have occurred, and was of small importance in a private memorandum of one, who well knew the quantities he had to work with: and this will appear evident from the manner in which the number (7.5686) is found.

Bradley had been calculating refractions for different zenith distances, and among others the observed quantity for 69° is estimated at 2′ 23″. At the end of this part of his work he has fortunately added (though he has not done so in other places) the following memorandum. "These observed refractions are collected from the observa- " tions made July 9th, 1750, (between 17^{h} and $18^{h}\frac{1}{2}$ sid. T.); the latitude being sup- " posed 51° 28′ 39″, and the error of collimation in the zenith −2″, but at the pole −3″ + " (and 4″ near the horizon to allow for the motion of the quadrant as the telescope " approaches the zenith.)" Now he did not begin to make regular entries of his observations with Bird's Quadrant till August 1750; but in the book of Observed Refractions there is an entry of several for this 9th of July, 1750; and it is stated that the barometer was at 30.04, and the thermometer 70°. Hence

tang. 0° 2′ 23″	6.8409110
tang. 69	9.4158226
	6.4250884 tang. of 54″.89 ref. at 45°
log. 30.04	1.4776999
	4.9473885
log. 348+70=418	2.6211763
	7.5685648

This last logarithm is exactly what Bradley has inserted in his rule, and there are numbers on the same page of his paper which leave no doubt, not only of his having arrived at it in this particular manner, but precisely from the refraction here taken of 69°. The process, when reduced to a formula, gives

$$\text{tang. } 54''.89\,\frac{b}{30.04}\times\frac{418}{348+t}\,\text{tang. }(\text{Z.D.}-3\,r)=\text{tang. of refraction.}$$

It appears therefore that in the rule we are to read "log. tangent," instead of "log." of refraction. In the same manner over log. 7.5686 he has written "12′ 44″=764" of which quantity his number is the logarithmic tangent, although that circumstance is not mentioned. It is not necessary to shew that Bradley understood his own rule, but it may be satisfactory to shew that we have not mistaken it by annexing the following example of his application of it.

" B. 29.9	7.5686 (constant logarithm)
Th. 31	1.4757 (log. 29.9)
75. 30	2.5786 (log. 379=348+31)
−12 (three times the refraction)	6.4657
75. 18	0.5811 (tang. 75° 18′)
	7.0468 (tang. 3′ 49″.7)
	3 50—"

The intimate connexion between this rule and the result of Simpson's investigation most probably did not escape Bradley's observation, but there are no traces of the particular steps by which the transition was made; and it is remarkable, that, with the exception of a few examples to each of the preceding rules, he continues to work on for many subsequent pages by the method, which he had first adopted. He probably trusted most to it for finding the quantities, by which he might establish the coefficients.

The correction for the variation of the barometer created no difficulty, but that for the alteration of temperature seems to have occasioned considerable perplexity. In the first place in which it is mentioned, the denominator has originally been $400+t$, which has been subsequently altered to $450+t$, which is the very quantity which Dr. Brinkley[m] found to answer best at low altitudes for Bradley's form of calculation, and this change was possibly connected with the memorandum which occurs at the top of the next page: "Machin's density is as $\frac{29.25}{432.8}$, and his elasticity as 432.8." This probably was the result of some actual experiments, for in another place we find

"Mr. Mach. greatest $\begin{cases}\text{diff. dens. as} \\ \text{elasticity as"}\end{cases}$

Unfortunately the numbers have been worn off the corner of the paper, or they might have afforded some means of estimating the care with which the experiments were made. Bradley however in this, as well as in the other parts of the inquiry, did not trust to any reasoning *à priori*. He referred every thing to his observations; and the manner in which he deduced the numbers for the thermometer, as well as the multiple of refraction to be subtracted from the zenith distances, may be seen in the Memoirs[n] prefixed to his Miscellaneous Works.

When calculating from the observations which were made nearly at the same time, he did not at first introduce the corrections for pressure and temperature into each, as all must have been nearly affected by them in the same manner: and it was from a comparison of the results, combined with a mean estimate of the barometer and thermometer for each set, that he calculated the modifications which it was necessary to introduce. This of course produced variations both in the logarithms, which at different times he had to apply to the sine of the zenith distances, as well as to the tangent of the observed refractions. These however he afterward endeavoured to remedy, as will be seen from the numbers, which are copied at the bottom of plate I. They serve likewise to shew (although further evidence is hardly requisite) how much Bradley's calculations were originally founded upon Simpson's method. It is shewn in the Mathematical Dissertations[o], that the sine of the observed distance is to be multiplied by $1-(h-k)$ to find the sine of an angle, the difference between which and the given zenith distance will be to the corresponding refraction as $1 : \frac{k}{h-k}$. Now k, as determined in p. 58, is 0.000253 or the refraction at 45°, expressed in parts of the circumference to radius 1; and h, which depends on the height of the atmosphere, is brought out by Simpson (in p. 59) to be 0.001643. Bradley assumes analogous quantities, though not exactly of the same value. His calculation is applied to the observation of α Cygni, which, on the 26th of

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[m] Trans. R. of Irish Acad. vol. XII. p. 104.

[n] P. lxxxviii.

[o] p. 58.

Oct. 1750, will be found to have had the apparent Z. D. 83° 58′ 5″; the correction for collimation would take off 4″, and the odd 1″ is neglected. 7.5825 is some modification of 7.5686, the exact elements of which it may not be easy to determine; but tang. $59''\frac{400}{30}$ will have for its logarithm 7.5814, and probably is not far from the truth. Hence

7.5825
+1.4694 log. 29.47 (barometer)
−2.5807 log. 380.8 (for thermometer 22)
6.4712 log. 0.00029594 tang. 61″ and log. k, which is written against it, evidently means the log. of Simpson's quantity.
4.7015
−2.5739 log. 374.9
7.2754 log. 0.00188539.

The first quantity (4.7015) is the log. of Simpson's h, which is probably taken for some standard number, which differs by 6 from that, for which the logarithm of k was adjusted.

Hence $0.00188539 = h$
$0.00029594 = k$
$0.00158945 = h-k$
$0.99841055 = 1-(h-k)$ which is Simpson's multiplier.

With these numbers an auxiliary angle is calculated precisely according to Simpson's method:

9.9993091 log. $0.99841 = 1-(h-k)$
9.9975877 sine 83° 58′ 0″
9.9968968 sine 83 9 31½
48 28¼

The whole may now be completed:

6.4712 was found for the log. of k
−7.2012 log. $0.00158945 = h-k$
9.2700 log. $\frac{k}{h-k}$
8.1492 tang. of the auxiliary angle 48′ 28″
7.4192 tang. 9′ 1″½ = refraction at Z. D. 83° 58′

The use, which Bradley therefore made of what he called z and x[p], are in this instance brought out exactly similar to the logarithms of Simpson's $(1-(h-k))$ and $\frac{k}{h-k}$. Though no instance has been observed among the earlier calculations of his making specific allowances for the barometer and thermometer in settling the logarithm, which was to be added to sines of his zenith distances, it is by no means improbable that some rough estimate determined him to increase or diminish the quantities with which he began his trials[q].

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[p] P. 7.

[q] For the observed refraction we have the following notice annexed to the second determination of the latitude, as quoted by Dr. Mas-

He was not a man to employ himself without consideration of the means most useful for the end which he had in view.

In estimating his temperature, Bradley at first appears to have depended on his thermometer, which was in the open air. This is evident from a memorandum in the beginning of his calculations. It says, " that in the hot weather, about the middle of July, " the thermometer by the clock stood at 75;" and it goes on to speak of " one out of " doors," which was considered to " have been about 80;" the barometer was about 30.00 at that time: hence it adds, " the density of the atmosphere was as $\frac{30.00}{450+80}$." In the same manner the height of the thermometer in doors is not annexed to any of the observations with Bird's quadrant before the 11th of Dec. 1750, although the instrument had been expressly hung on the western side of the pier with a view to making observations with it for determining the latitude and the laws of refraction. From what Mason says[r], it seems, however, probable, that Bradley afterwards used the mean of the thermometers within and without; and this agrees with the entries in the book of " Observed Refractions." To each of the observations in 1750, several of which are used, there is but one notice of a thermometrical height; and by a comparison with the observation-book, it is clear that this was the height out of doors. But to the observations of 1751 and 1752 there are annexed the heights of the mercury both within and without. In one instance the calculated refractions are set down opposite to those which were observed; but this only occurs for Nov. 17, 1750, and consequently gives no means of examining the share, to which the heights within doors were afterwards admitted, in framing the calculation.

Bradley's thermometers have often been inquired for; but they are most probably no longer in existence. All that are now to be found at Greenwich are of a later date. And this very circumstance makes it desirable to correct a mistake (notwithstanding its small amount) which has occurred with respect to one of those which he used.

When Dr. Hornsby published the Greenwich Observations, he introduced two separate columns into each page for the quadrant, and inserted in them the heights of the thermometers within and without. The manner, as will be presently explained, in which the entries were originally made was somewhat different; and to accommodate the text to the alteration, he has varied the arrangement of a notice which occurs in 1750. This in itself would hardly be worth mentioning, if he had not made a little oversight in reading the numbers which it contains.

Between the 21st and 24th of October, Bradley has entered the following memorandum in the north quadrant book: " The thermometer here noted was made by Mr. " Sisson. Oct. 22, I immersed it in snow, and found the mercury sunk only to 33—or " 33, so that it stands near 1° too high. That made by Bewley sunk exactly at the " same time and place to 32. Subtract 1° from Sisson's to have the true number."...This was originally " 33— or 32½, so that it stands near ¾° too high;" but 32 has been evi-

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kelyne: "Mem. Assume a mean refraction, and " correct the observations thereby from the bar. " and ther. both at the pole and equator, and " thence may be collected the error of the as- " sumption."

[r] Memoirs, P. lxxxix.

dently altered to 33, and the fraction ½ has been scratched out, although imperfectly. Now Bradley's own description is very clear. The mercury sunk a little below 33, and he at first thought that the heights which it indicated might be considered at 0,75° too great; but he afterwards saw reason to think that the depression below 33 did not amount even to 0°.25; and as it seemed not worth while to take so small a difference into account, he was induced to call the reduction an even quantity of 1°. On the other hand, " 33— or 33½," as it stands in the printed text, will infer a doubt to the extent of more than 0°.5, which could hardly have occurred to so careful an observer[r]. Accordingly M. Bessel[s] very justly concludes, that the thermometer " temperaturam nivis liquentis in 33°.25 indicavit," whereas the 33° was the very utmost height for Bradley's thermometer out of doors; and there appears to have been neither excess nor deficiency in that which he kept within.

On the 11th of Dec. 1750, we find in the printed text a note that " the thermometer " placed within was made by Bewley," with a reference to the memorandum in the preceding October. This is all very clear, and suits the form given to the publication better than the exact entry as it stood in the original book. In the thermometer column we there find on the day which has been stated, "Th. 45 S.;" and after the name of the pole star is added, "Th. 46. Bewley." Under the whole Bradley has written, "Th. "S. is without doors, and Th. B. is within doors." In the MS. "Th. S." continues constantly to occupy the thermometer-column, which corroborates the idea of Bradley's having originally considered it to be the fundamental quantity; and "Th. B." is almost always annexed as an addition after the name of the star to which it refers. The designations of "Th. S." and "Th. B." are continued for the subsequent quadrant-observations, and are used likewise for the "Observed Refractions." It would have been very inconvenient to have retained this form in the printed text, and much trouble was saved by the alteration to readers, who must otherwise have been continually under the necessity of recollecting the position of each instrument; but the entries in the manuscript lead us to some particulars of which we might otherwise have found no indication. In the calculation book we had the mention of a thermometer by the clock; and it is not improbable that Bewley's is here referred to: it is possible that it was fixed in the clock-case. Again, the same descriptions of "Th. S." and "Th. B." are used in the south as well as in the north quadrant book. It is clear, therefore, that the temperatures were entered from the very same thermometers for the observations, which were made with both the instruments.

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[r] M. Deluc (Mod. de l'Atm. vol. ii. §. 438. h.) points out particularly that melting snow is an accurate measure of the freezing point, and, although Bradley does not specifically mention it, we may fairly infer, with M. Bessel, that the snow was in that state. If it had been cooled down below that temperature, the mercury could not be supposed to have stood so high in it, without Sisson's having erred, more than can well be imagined, in his graduation. But there is no occasion for recourse to any original error. If it was not new, it may have undergone a material alteration. M. Flaugergues detected that the freezing point in mercurial thermometers is subject to a rise, which gradually advances for some considerable time after their original construction. Bradley had a thermometer of Bird's, (Memoirs, P. lxxiv.) and it is remarkable that he did not use it for his refractions. There can be no doubt of its having been constructed with all the peculiar care and accuracy of that great artist, and he seems to have paid particular attention to his graduation. Lord Macclesfield made the following entry in his observation-book at Shirburn : " 1756, March 23. 0. 15. Clouds with much " snow all the morning till just now. I this " morning tried Bewley's thermometers in thaw- " ing snow. That which stands without doors, " and is Fahrenheit's scale, stood ¼ of a de- " gree above the freezing point. That which " stands just within the door, and is D'Lisle's " scale, stood ½ a degree above the freezing " point. I afterwards tried Mr. Bird's large " thermometer, which hangs near the clock, " and in the thawing snow it stood precisely at " the freezing point." In this case, however, the thermometer might not have been made long enough to have undergone any alteration. That which was constructed by the same hand for Greenwich was broken a short time after Dr. Maskelyne's death. There is, however, one of Bird's thermometers in the Observatory at Oxford. This has been immersed to the top of the mercury in pounded ice, which was suffered to melt slowly in a room where the temperature of the air was between 34° and 35°. The vessel of ice was sufficiently large, and the experiment was continued for a considerable interval; but the mercury stood steadily, and as precisely as possible, at 32° of the scale, which is about 0.1 or 0.2 of a division below the mark which the maker had filed on the tube for the fixed point. From a letter of Bird's to Dr. Hornsby it is probable that this thermometer was made in 1772; but as Bird died in 1776, it must have been constructed more than half a century. M. Flaugergues attributes the alteration to a contraction of the bulb from the pressure of the external air. How far the strength of the Oxford thermometer may have been calculated to resist such an effect, may be estimated from its dimensions: the tube on the outside is 0.17 of an inch in diameter, and the end is blown out into a bulb whose diameter is 0.52.

[s] Fund. Ast. p. 28.

ACCOUNT

OF

HARRIOT'S ASTRONOMICAL PAPERS.

AT the death of the celebrated Thomas Harriot in 1621, his manuscripts became the property of Henry, earl of Northumberland, in whom he had found a most munificent patron during the latter part of his life. The male line of the ancient Percies ended with the earl's grandson Joceline, from whose daughter the earls of Egremont are descended. By this means most of the manuscripts passed into their possession[a]. The value and curiosity of these papers were known, and Count de Bruhl pointed[b] them out particularly, in 1784, to the notice of the Baron de Zach, who immediately called public attention to the subject[c], and formed a plan for collecting and printing a new edition of Harriot's works. In the astronomical ephemeris of the Royal Academy of Berlin for 1788[d], he inserted a general account of the papers, dwelling more particularly on the astronomical observations which they contained. This, having been translated, was separately printed for circulation in England, and a copy has been subjoined to these remarks[e]. I shall have such constant occasion to consider the several parts of it, that it seemed endless to note them: when any expression therefore of the Baron's is quoted, without a distinct reference, it may be generally understood to be taken from this document. It seems to have been drawn up while he was yet uncertain of finding any one who would engage in the heavy expense of his intended publication; but in 1786 he applied to the university of Oxford for assistance in printing it. This was immediately acceded to, and in April of the same year he wrote a long letter, containing details of his plan, to the delegates of the press[f]. He promised a life of Harriot after the manner of Gassendi, which, with the original observations of comets that had been seen in 1607 and 1618[g], was to make up the first volume of his intended work. On the receipt of this specification an order was made for the printing to proceed as soon as the editor was ready, but there is no record of his having advanced any further in the undertaking. He was indeed appointed in 1786 to the care of the duke of Saxe Gotha's observatory at Seeberg; but this had taken place previous to his application to the university. However, in 1793 he inserted in the Supp. to Bode's Jahrbuch the observations of the comets of 1607 and 1618. These were accompanied by notes, which seem to indicate that they had been drawn up with a view to his intended publication at Oxford; and if so, they fix the time when he had abandoned this intention. I am inclined to believe, though I have no direct proof it, that by lord Egremont's indulgence a portion of the papers were all this time in his keeping; for in May 1794 he transmitted to Oxford, by bishop Cleaver, then principal of Brazennose college, a certain number of the manuscripts, though without any of the apparatus, which he was to have drawn up. It is now clear, though it does not appear to have been then known, that this was only a small part of the whole; and as it will be seen that such a selection had not been made as to render the several parts complete in themselves, it seems most probable that they had been taken out at first for use in the conduct of his work. It may easily be understood that the Baron might not be very scrupulous in troubling himself at once to take every thing, which he might require, when a further revision, as he went on, would enable him to complete what he might find to have been omitted: but it can hardly be conceived, that with the whole collection before him, he would have selected what he intended for Oxford in such a manner as to leave several essential parts either imperfect or unnoticed.

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[a] In the Savilian Library there is a copy of the Artis Analyticæ Praxis, which once belonged to Sir Charles Cavendish, of whose conversation with Roberval, Wallis (Opera, vol. II. p. 204) tells the remarkable anecdote which has so often been repeated. The book was Wallis's, and he has written several particulars on the fly leaf, which he concludes by saying that Harriot had left many other "worthy "pieces; . . .some of them I have seen. But in "whose hands they now are, or whether they "be since perished, I cannot tell (March 27, "1677.)" The papers here alluded to may have been those which were in Dr. Pell's possession; for there are some MSS. of Harriot's in the British Museum, which seem formerly to have belonged to him, (6001, 2, 6083 Harl.). Lord chancellor Clarendon in 1662 had "some "considerable papers of Mr. Harriot," which are said to contain, among other things, "considerable observations on the weather." Birch's Hist. of the Royal Society, vol. I. pp. 120. 126. 309.

[b] See note A at the end of this account.

[c] The Baron says (Corr. Astron. vol. VII. p. 105) that he printed a little dissertation at Oxford about 1784, which gave an account of the contents of these papers, but I have never been fortunate enough to meet with it.

[d] Printed about 1785.

[e] See note C. This translation has not the appearance of having been printed at Oxford, and therefore is probably not the dissertation mentioned in note[c].

However this may be, the business had now assumed a totally different character. The delegates of the press had no longer to deal with an editor whom they had undertaken to assist in the publication of his work, but whatever was done with the papers must have been executed entirely on their own responsibility. It became necessary to inquire further, and ascertain the nature of the materials which had been put into their hands. It was therefore determined to obtain the opinion of some competent judge; and Dr. Robertson, then Savilian professor of geometry, having reported[h] that it would not be advisable to proceed with the proposed publication, the papers were, after some accidental delays, restored in 1799 to the earl of Egremont.

This is the plain state of the case; but a very different version of it has been circulated for many years. Dr. Hutton in his Dictionary (art. Harriot) has reprinted the translation of the baron's communication to the Berlin Ephemeris, and has added[i] [1796] that the papers were in a train for publication, having been given on that condition to the university of Oxford; the baron repeats this story of the gift[k], and Dr. Hutton retained it in the second edition of his Dictionary, which was published in 1815. The general impression has therefore been, that the baron having discovered these papers, lord Egremont made at once a present of them to the university, in order that they might be printed, and that to the injury of science they have been kept at Oxford in obscurity, without any regard to the condition on which they were accepted. This certainly is most erroneous. There was no call on the noble owner to give up his property in the papers, and it was most liberal in him to suffer the baron de Zach to transfer what he had kept under his own consideration for ten years, and submit any of the manuscripts to the university. There can likewise be no doubt that in the same spirit of liberality he would immediately have permitted the whole collection to have been used for the same purpose; but it is equally certain that only a small part was ever at Oxford. This portion, after it was returned to his lordship, was carefully preserved at Petworth, and answers so exactly to the description which is given in Dr. Robertson's reports, that no doubt can exist of its identity. The larger quantity, which has before been alluded to, (on a rough estimate about seven parts in eight of the whole,) remained at lord Egremont's house in London, and were given by him in 1810 to the British Museum. These of course have been examined. Several important documents have been found among them, but in occasional and short visits to the metropolis it was impossible for me to study the contents of all with minuteness. To follow out such an inquiry it is necessary to compare the mutual bearing of the several parts, when the particular topics that they embrace are each fresh in the recollection; and this could only be done by one, who could possess the uninterrupted opportunity of referring to every thing exactly as the doubts or their explanations happened to occur. Lord Egremont's indulgence has however enabled me to follow out one part of the subject with tolerable completeness. The baron de Zach's first object seems to have been the exhibition of Harriot in the new light of an astronomer; this was probably the reason for his having taken out and sent to Oxford the most important papers relative to that point. It will be seen that he omitted some which are necessary to the right understanding of Harriot's claims; but there seems to be no other obvious way of accounting for his having communicated so very little comparatively upon other subjects.

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[f] See note B.

[g] Whenever this date is mentioned, it must be understood with reference to the third comet of that year: Pingré Cometographie, vol. II. p. 8. Halley's comet is in the same manner intended to be designated when that of 1607 is referred to.

[h] See note D for these reports.

The baron, in his account of Harriot's observations, has not arranged them chronologically, which is of the first importance for the consideration of claims to discovery, and with this view they must be referred to five distinct objects. I. Halley's comet, when it appeared in 1607; II. the moon; III. the satellites of Jupiter; IV. the spots on the sun; and V. the comet of 1618.

Of I. and V. an accurate opinion may be formed from what has been printed in the appendix to Bradley's works[l], combined with the further remarks which will be hereafter submitted to the reader, when we come to consider Harriot's view of the orbits of comets.

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[i] See note E.

[k] Corr. Astron. vol. VII. p. 105.

[l] It may be right to correct a mistake of "ceruso" for "cerusa," in P. 520. "Cerusa scripta" is indeed not a very precise expression for what it was intended to describe; but the

II. The observations of the moon contain one as early as July 1609. According to professor Moll's curious and valuable account [m], telescopes had been made in Holland in the latter part of the preceding year; but Galileo did not construct one before May 1609 [n]. It is probable, therefore, that Harriot had his directly from the first inventor. No object was more likely to have drawn early attention than the moon; but the drawing made of what presented itself was extremely rough, the moon was "5 days old," and the edge of its illuminated disc is serrated at random, with only an attempt in one place to make some little of an outline. Subsequent observations made between July 17 and Sept. 15, 1610, present drawings of the moon at different ages; but the Sidereus Nuncius had then been published, and it was probably in pursuing Galileo's discoveries that the following memorandum was subsequently entered on the 23rd of October:—"I "observed that the line of division betwixt the light and the shadows passed by the "outward side of Caspian, and it shewed mountenous. By this observation it may "appeare how farre it is from the outmost perifery of the moone."

One of the objects to which Harriot seems to have paid particular attention, was the endeavour to determine by observation the time when the moon was dichotomised. This was possibly with a view to the method suggested by Aristarchus for finding the distance of the sun from the earth; but nothing like precise certainty was obtained in the result. Thus we find on April 9, 1611:

"10^h 5′ not yet a right line.
10 20 very nere, but yet not perfect.
10 45 yet doubtful to be perfect.
10 52 as I judge now unsensibly a right line, one accidentall rag or two at and nere the lower corner being abstracted. but others say not yet perfect.
11 15 yet a right line and not contrary. but if not right rather wanting by the lower corner.
11 30 yet continuing. others say wanting.
12 0 } all say it is a right line. At 12^h.0 I described the appearance as soon as I could
12 15 } by my instrument of $\frac{32}{1}$."

The observation is dated at Syon, and Harriot annexes the following memorandum: "Sir Nicholas Sanders [o] and Christopher were with me, and also observed in my "garret."

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baron has used it again in another place. Harriot's observations of both the comets were originally noted down with a black lead pencil, and the writing has been traced over with ink. This is what is alluded to in the memorandum where the correction has just been pointed out, and at the end of the whole we find again, in the baron's handwriting, "N.B. Hæc cerusa "notata erant, ego attramento παντογραφῇ· punc"tulis notata difficile erant lectu."

[m] Journal of the Royal Institution, vol. I. p. 319.

[n] "Mensibus abhinc decem"—he was writing in March 1610.

[o] Sir Nicholas Sanders was member for Maidstone in 1585, and for the boroughs of Penryn, St. Ives, Helston, and Lestwithiel, in the four last parliaments of queen Elizabeth. He appears to have been knighted on the accession of James I. and was member for Gat-

There are two drawings of the full moon, with a table of references noting the relative situations of the principal points on them. They do not differ from each other so as to shew any effect of libration, and the figure given to the less brilliant parts is of course only a first approximation; but it must have required considerable time and attention to construct what possibly were the very first maps of the kind which were ever attempted to be drawn.

The baron de Zach mentions "some eclipses," and of course a careful search was made for them. There was one half sheet of paper, on which circles had been drawn to represent the phenomenon, when it was either annular or extending to a certain number of digits; and there was another, on which was inscribed "anno 1601, Dec. 14, eclipsis "⊙;" but there is also written on it "Tycho, pag. 132;" and in that very page of the Progymnasmata the calculation of it will be found; but there was no observation even of T. Brahe himself, for he died in October 1601. Harriot has examined this calculation; but no record whatever was found of any eclipse either of the sun or moon which he had observed himself.

III. By far the most curious and interesting part of the astronomical papers are connected with the early observations of Jupiter's satellites. The records which we have of Harriot's labours on this point consist in his drawings of the configurations which he observed, and the calculations which he made to determine the revolutions of these newly discovered bodies.

There are fourteen half sheets of foolscap filled with the representation of the appearances which he noticed. One of these contains the rough entry of a few of the earlier observations, and the other thirteen are filled with more regular delineations. The whole are clearly written, and a fair estimate of them may be formed from the facsimile (in plate II. Supp.) The upper part contains the rough entry of his first observation, and the lower part of the plate represents the beginning of the fair copies which he afterwards wrote out. Such a specimen as this seemed on many accounts to be more satisfactory than any attempt to describe them; but as the handwriting may present some little difficulty, it will not be superfluous to add in this place a copy of the entries which it expresses.

"My first observation & others following of the new found planets about Jupiter.
1610 Syon.
1. Octob. 17 ☿ ho. 12^a, 1^a, 2^a. I saw but one, & that alone.
   Blackfriers, London.
2. November 16 ♀ ho. 9^a, 10^a. I saw one fayre 9′ or 10′ above, and sometimes I thought I saw an other very small betwixt them 3′ or 4′ a ♃.
   London.
3. November 19 ☽ ho. 9^a one under fayre.
   Syon.
4. November 28 ☿ ho. 9^a one under fayre.

ton in 1603. From the frequent appearance of his name in committees he must have taken a very active part in the business of the house.

5. November 30 ♀ ho. 9^a one above fayre.
6. December 4 ☿ ho. 9^a one under fayre.
7. Decemb. 7 ho. 9^a, $9^a\frac{1}{2}$, I saw but one, & above.
8. Mane ho. 17^a. Two seen on the west side, a little under. Sir W. Lower also saw them here. The nerest fayrest. The farther not well seen within the reach of my instrument of $\frac{20}{1}$ of 14′ dyameter."

Harriot speaks of "the new found planets," and says that in his first observation he "saw but one:" it is evident, therefore, that he knew of there being more, and had looked for them in vain. It is indisputable, likewise, that he did not even succeed so far till October, 1610, nine months after Galileo. The cause of this delay, and the other circumstances connected with it, will be completely made out hereafter. For the present it will be best to confine our attention to the observations themselves. They were made, for the most part, at Syon house, the seat of the earl of Northumberland, on the banks of the Thames near Isleworth; and we find accordingly, that he notes distinctly on Nov. 16, his having removed to London, and on Nov. 28 his having returned to his usual residence. The baron de Zach was clearly mistaken in his opinion of Harriot's having observed at Sion college; but he might naturally be led to this conclusion by finding it said in Antony Wood, that Harriot "for some time lived in Sion coll. near to "London[p]:" the thing however was impossible: as Wood was an antiquary, he must have known that Sion college was not founded till after Harriot's death, and that it was not near, but in London. He was in the habit of writing in abbreviations, and "Sion "ho." was probably mistaken by the printers for "Sion Co." Harriot, from what he says in his observations of the moon, appears to have had an upper room of the mansion especially assigned to him for his astronomical pursuits.

The several observations of Jupiter's satellites are regularly numbered up to 90, which are in a uniform series on to May 28, 1611; there are also three made Oct. 1, Oct. 6, and Dec. 11, of the same year, and six made in January and February 1612. These make together ninety-nine. There are likewise nine from April 25 to May 5, 1611, which are described as having been made "in a gutter of a house" in London, by Christopher, who appears to have been Harriot's regular assistant. These are not numbered in the series, and Harriot shews himself particularly careful to distinguish what he had seen himself, and whether his observations were made alone, or in company with others. Thus in all his first descriptions of Jupiter's satellites he uniformly says, "I saw," till the 14th of March, 1611, when he possibly began to have the assistance of Christopher; and from that time, with comparatively few exceptions, he speaks of what "we saw." Nor is this accidental, for in entering the observations of the solar spots, he had originally written, in 1612, Jan. 6, 19, "I saw," &c. but he afterwards inserted "we" over the lines.

The first time, on which he was able to see the four satellites, was on the night of Dec. 14, 1610, "all clere and seen at once:" he has written over this entry ("omnes

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[p] Athenæ, Oxon. vol. II. p. 303.

"in recta linea cum asello australi. ut proxime)[q]." He has inserted an additional memorandum, in the same place, and says, "hora 8^a circiter the same night ♀ "being $\frac{3}{4}$ gr. high did eclipse ♄. 2 houres before I found them by my trunks "$\overset{10}{\mathrm{T}}$ $\overset{20}{\mathrm{T}}$ about 6′ or 7′ asunder. So that I think about $\frac{1}{2}$ an houre after they were "nere centrally conjoined." The square wooden spouts which are used to convey water on the outside of houses are still called by the workmen "trunks," and Junius says in his Etymologicum Anglicanum, "Trunk, sipho, siphon, similiter a Theot "trunc, quod nihil aliud est quam stipes equali foratu in longum excavatus." The name may have obtained at first from the glasses being set in square tubes. Galileo used a leaden pipe (tubum plumbeum): Sirturus complains (cap. XIX.) that "artifices "& imperiti,...usi sunt tubis ex lamina ferrea & cartaceis," and that "angustum & con- "tinuo æqualem producebant ignorantes visionum & radiorum naturam." This will account for the name of "cylinders," by which we shall also find the instruments designated. The print of Simon Marius in his Mundus Jovialis (1614) represents a telescope lying on the table before him, which is not cylindrical, but larger at the end in which the object glass was inserted than at the other. The perspective is not to be depended on, but the instrument appears to be about 18 or 20 inches in length, and the larger end about an inch in diameter[r]. Of the size of Harriot's we have no precise description, but he mentions $\overset{20}{\mathrm{T}}$ and $\overset{10}{\mathrm{T}}$, for the magnifying powers of twenty and ten, which belonged to the two, which he refers to as being used for the observations of Jupiter's satellites. Telescopes from the first were applied to terrestrial objects, and therefore most probably were then fitted with concave eye-glasses, analogous to what is commonly called the Galilean construction: Harriot's appear to have been of this kind, for the images which he represents are not inverted. Jupiter was near its opposition in December 1610, it therefore was in the east at night and in the west in the morning. Now on the 17th of December we find the two satellites which were on the west, not only drawn on the right hand of the planet, but declining also downwards to the right, in their really apparent position; whereas, all the preceding observations taken at night exhibit the line of the satellites inclining downwards to the left. By describing them to be "fair," he means of a good size and distinct: thus on Jan. 26, 1611, he says, "I saw three. Two occi- "dentall fayre. The nerest to ♃ the fayrest. That orientall small, but well seen." And the spots on the sun are constantly spoken of as "fayre and black." His great criterion of the clearness of the night and of the effect of his telescope, was the nebula in Cancer. He is constantly referring to it. Thus 1610, Dec. 27, a satellite, which had been marked as fayrest of the whole four in the preceding morning, was not visible; "yet præsepe was wel seen, & by the instrument his starres." On the morning of Jan. 21, 1611, he saw two satellites, one "fayre. The other small, & but sometimes

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[q] Another similar notice occurs in 1611, "Oct. 6. I saw sometimes two, and certainly. "The ayer was variable. one occidentall the "fayrest about 5′ of ♃. The other orientall "about 3′ of. All were in a right line with "cor ♌ and Asellus Australis."

[r] In the engraved title to Hevelius's Selenographia (1647) there is the representation of a telescope, which, when compared with the height of the man who holds it, must be between five and six feet in length, and of a proportionate diameter: it consists of five cylindrical joints, which slide, the smaller into the larger, exactly as they are now constructed.

"seen. præsepe not seen, & the moone not farre off, shining bright;" and on the night of the same day he could distinguish none of the satellites, "starres cleare, præsepe hardly "seen. I saw nothing, though I looked diligently." The same disappointment occurred to him on the 30th of March, "starres cleare, ♃, other starres and præsepe wel seen, "yet no Joviall by any of our instruments."

Estimates are annexed of the apparent distances of the satellites from Jupiter; these are generally given in minutes and half minutes, in one or two instances we find ¾ of a minute, and on the morning of Dec. 18, 1610, he has entered that he saw one as he "thought 10″ or 15″ à ♃, hardly seen." He gives his measures therefore from the limb and not from the centre of the planet; and accordingly there is a rough entry of the observation of Nov. 28, 1610, to which is added "1′½ à ♃, or diameter Jovis "à ♃." This is the same observation, which he has written out as the fourth in the fair copy, and he has there made the distance 2′. In fact his manner of estimating these distances did not admit of certainty to very small quantities. Galileo, in the beginning of his Sidereus Nuncius, explains, that by covering his object glass with plates, which were perforated with circular apertures of different dimensions, "angulos alios atque "alios pluribus paucioribusque minutis subtendentes pro libito constituemus, quorum "ope stellarum intercapedines per aliquot minuta ad invicem dissitarum, citra unius aut "alterius minuti peccatum commode demetiri poterimus." By this means he took his distances to ⅓ of a minute: he says indeed on the 8th of February, "stella Jovi proxima "illum fere tangebat, distabat enim ab eo min. o. sec. 10 tantum," but this must have been an estimate, not a measure. Harriot's quantities were more roughly taken. He gives no reason to think that he had made any previous experiments like Galileo, who determined the value of his apertures by the length which they included of a line at a given distance. One method which Harriot used has been already mentioned, which was to take the diameter of the planet itself for the standard; and so on the 16th of March 1611 he describes one of the satellites as being "about 5 diameters of," which, on the configuration is called "9′ or 10′." But his most general way depended on the relative distances when compared with the places which the bodies occupied in the field of his telescope. In the observation of the 17th of December, 1610[s], the satellite was said to be "not well seen within the reach of his instrument," the field of which he describes as being 14′ in diameter; and on the 25th of January, 1611, a satellite under similar circumstances is set down at 12′ or 13′ from Jupiter. In both these cases he used his telescope $\frac{20}{1}$, but the diameter of its field was not very precisely determined, for among the observations of the spots on the sun, the following memorandum occurs; 1612, July 16th, "with $\frac{20}{1}$ I see little more than ⅓ of the diameter of the sonne. That is 11′ or 12′." Such estimates must of course be very loose: in one of the observations of the dichotomised moon we find "anno 1612, October 21 ☿ ho. 6°. post M. The division then "betwixt the light & darknes was in a right line observed by my trunke of $\frac{20}{1}$.... At "that time the rounde valley *a* (fig. 1. plate IV. |Sup.) was ⅓ from the lower point, & "⅔ from the higher, because *ab* was the diameter of my instrument, & *ac* semi-"diameter."

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[s] P. 22.

It is clear that such quantities are not to be depended upon. Harriot estimated the diameter of Jupiter at something less than 2′. This is not to be wondered at; neither is it to be attributed to him as a fault. His telescopes probably did not represent the objects with sufficient sharpness for their outlines to be properly seen, and the blaze of light would naturally have increased their apparent magnitudes[t]. It may be said that the distances of the satellites deduced from this standard are relative, and therefore may be corrected accordingly; but we have no very definite estimate of the standard, and there are comparatively few instances in which there is a note of its having been used. When the field of view has been taken for the micrometer, we have no means of making allowance for the errors in it, or to correct the quantities which Harriot has in this manner deduced from his observations. The roughness with which he observed is clear in another circumstance: Galileo discovered the satellites on the 7th of January, 1610, and on the 13th he says, "primum a me quatuor conspectæ fuerunt stellulæ.... tres occi-"dentales & una orientalis: lineam proxime rectam constituebant; media enim occi-"dentalium paululum a recta ad septentrionem versus deflectebat." He afterwards notices repeatedly that a satellite was out of the line; but there is no indication of any thing of the kind in Harriot's text or drawings. On the contrary, the only passage in which any allusion is made to the fact is quoted above[u], and he there mentions them as being "omnes in recta linea."

To have published a collection of such observations could have been of no use, and the expense of engraving all these configurations would have been a waste of money. The papers, however, are of singular curiosity, from containing, in all probability, the very earliest original records in existence of any observations of Jupiter's satellites. They furnish us likewise with several particulars connected with the history of Harriot's observations, which are remarkably interesting; but the account would be very deficient without the explanations that are found in that part of the collection which has been deposited in the British Museum. Much light, for instance, is derived from the following letter:

"To his espesial good friend Mr. Thomas Harriot, at Sion neere London.

"I gave your letter a double welcome both because it came from you and contained newes of that strange nature; although that which I craved, you have deferred till another time. me thinkes my diligent Galileus hath done more in his threefold discoverie than Magellane in openinge the streights to the South Sea or the dutchmen that weare eaten by beares in Nova Zembla. I am sure with more ease and saftie to him selfe & more pleasure to mee. I am so affected with his newes as I wish sommer were past that I mighte observe the phenomenes also. in the moone I had formerlie observed a strange spottednesse al over, but had no conceite that anie parte therof mighte be shadowes; since I have observed three degrees in the darke partes, of which the lighter sorte hath some resemblance of shadinesse but that they grow shorter or longer I cannot yet perceive. ther are three starres in orion below the three in his girdle, so neere togeather as they appeared unto me alwayes like a longe starre, in so much as aboute

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[t] Jupiter's "apparent diameter (measured "equatorially) at his mean distance from the "earth is 36″.74. At its conjunction it is "sometimes only 30″.0; but it increases as "the planet approaches its opposition, when it "sometimes amounts to 45″.88." Mr. Baily's Astronomical Tables and Formulæ, p. 36.

[u] P. 23.

4 yeares since I was a writing you newes out of Cornwall of a new a strange phænomenon but asking some that had better eyes then my selfe they told me, they were three starres lying close togeather in a right line. thes Starres with my Cylinder this last winter I often observed, and it was longe er I beleved that I saw them, they appearinge through the Cylinder so farre & distinctlie asunder that without I can not yet dissever. the discoverie of thes made me then observe the 7 starres also in ♉, which before I alwayes rather beleved to be 7 then ever could number them. through my Cylinder I saw thes also plainelie and far asunder, and more then 7 to, but because I was prejudgd with that number, I beleved not myne eyes nor was carefull to observe hou manie; the next winter now that you have opened mine eyes you shall heare much from me of this argument. of the third and greatest (that I confesse pleased me most) I have least to say; saving that just at the instante that I receaved your letters we Traventane Philosophers were a consideringe of Kepler's[x] reasons by which he indeavers to overthrow Nolanus & Gilberts opinions concerninge the immensitie of the spheare of the starres and that opinion particularlie of Nolanus by which he affirmed that the eye beinge placed in anie parte of the univers the appearance would be still all one as unto us here. when I was a sayinge that although Kepler had sayd somethinge to most that mighte be urged for that opinion of Nolanus, yet of one principall thinge he had not thought; for although it may be true that to the ey placed in anie starre of ♋ the starres in Capricorne will vanish, yet he hath not therfore so soundlie concluded (as he thinkes) that therfore towards that parte of the world ther wilbee a voidnesse or thin scattering of little starres wheras els round about ther will appeare huge starres close thruste togeather: for said I (havinge heard you say often as much) what is in that huge space betweene the starres and Saturne, ther remaine ever fixed infinite numbers which by reason of ther lesser magnitudes doe flie our sighte, which may supplie the appearance to the eye that shalbe placed in ♋. what if aboute ♄ . ♃ . ♂ &c. ther move other planets also which appear not. just as I was a saying this comes your letter. which when I had redd, loe, q^{d}. I what I spoke probablie, experience hath made good; so that we both with wonder and delighte fell a consideringe your letter. we are here so on fire with thes things that I must render my request & your promise to send more of all sortes of thes Cylinders. my man shal deliver you monie for anie charge requisite, and contente your man for his paines and skill. send me so manie as you thinke needfull unto thes observations. in requitall I will send you store of observations. send me also one of Galileus bookes if anie yet be come over and you can get them. Concerning my doubte in Kepler, you see what it is to be so far from you. what troubled me a month you satisfyed in a minute. I have supplied verie fitlie my wante of a spheare, in the desolution of a hogshead, the hopes therof have framed me a verie fine one. I pray also of your leasure answere the other pointes of my last letter concerning Vieta Kepler & your selfe. I have nothinge to presente you in counter, but gratitude with a will in act to be usefull unto you and a power in proxima potentia; which I will not leave also till I have broughte ad actum. if you in the meane time can further it, tell wher in I may doe you service, and see how wholie you shall dispose of me.

"Your most assured and loving
"friend,
"Willm Lowër."

"Tra' venti the longest
day of, 1610."

We shall hereafter have a better opportunity of enlarging on the author of this letter[y], and may therefore at present confine our attention to the contents of it.

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[x] "p. 106, de nova stella Serpentarii."

[y] See note I.

The Sidereus Nuncius contains observations on the inequalities of the moon's surface, on stars which cannot well be separated by the naked eye, and on Jupiter's satellites. These are precisely the "threefold discoverie" alluded to; and it is remarkable that the writer had before noted the singular appearance in the sword of Orion, although neither he nor Galileo were able to distinguish the nebula, which Huyghens afterwards discovered in this part of the constellation. There is something very striking in the simple, but animated account of the news arriving just at the moment when the party were discussing the possibility of secondary bodies moving unseen about the superior planets—an idea which Harriot had thrown out from analogy. From the tenor of the letter it may be concluded, that the fame of Galileo's discoveries had reached England before the work in which he gave an account of them, and that Harriot had derived the information which he sent to his friend from some other quarter. The dedication of the Sidereus Nuncius to Cosmo de' Medici is dated "4 Idus Martii," and by "5 "nonas Maias" Kepler had published his "Dissertatio cum Sidereo Nuncio." Now Harriot was in correspondence with Kepler, and studied his work de Stella Martis with the greatest attention, repeating and often correcting the calculations as he went on. On one sheet of these the following memorandum occurs:

("1610. Oct. 6 differt a calculo pruten 3° gr.
In dissertatione cum nuncio sidereo, p. 5.")

This has been evidently added after the rest was written, and the passage occurs in the given page of the first edition of Kepler's book; it is not improbable therefore that the news reached Harriot through these means before the original narrative had found its way from Italy. It was not much longer, however, in reaching this country: for sir Christopher Heydon, in a letter dated July 6, 1610, tells his friend Camden, "I have "read Galileus, and, to be short, do concur with him in opinion; for his reasons are "demonstrative: and of my own experience, with one of our ordinary trunks, I have told "eleven stars in the Pleiades, whereas no age ever remembers above seven, and one of "these, as Virgil testifieth, not always to be seen[z]." However this may be, it is perfectly clear that Harriot and his friend had been in the habit of using telescopes before the discoveries of Galileo were known to them; and it appears likewise that in 1610 they were manufactured in England. Sir William Lower's request for "more of all sortes of "thes cylinders," seems likewise to imply that they were multiplied at no very expensive rate. It is not easy, upon any other supposition, to account for the requests not being rather directed to the quality than to the quantity of the instruments.

The discoveries of Galileo seem to have been received with enthusiasm. The first edition of Kepler's Dissertation was so soon exhausted, that he published a second in the following year. With his accustomed warmth and eagerness he describes his own ecstasy when the news first reached him from Italy: "Quod cum .... Wackherius ... "de curru mihi ante habitationem meam nunciasset tanta me incessit admiratio absur-

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[z] Camdeni Epistolæ, p. 129. The baron de Zach refers to this passage, (Corr. Astr. vol. VII. p. 123,) but only for the mention of "ordinary trunks."

"dissimi acroamatis consideratione, tanti orti animorum motus (quippe ex inopinato de-"cisa antiqua inter nos liticula) ut ille gaudio, ego rubore, risu uterque ob novitatem "confusi, ille narrando ego audiendo vix sufficeremus." Sir William Lower does not seem to have felt less delight; and we therefore learn with regret that he was disappointed in his expectations of observing. Another of his letters in the British Museum is dated from Traventi, March 4, 1611, in which he says,

> "Concerning the Joviall starres I writte nothinge of them last, because I had nothinge to write. for indeede although both I and the young Philosopher att Haulesbrooke have often and in verie cleare nights (when Præsepe was most plaine to be seene without the cylinder) when wee I say have often diligentlie observed jupiter wee could never see anie thinge. I impute it to the dullnesse of my sighte for onlie with your greate glasse I could se them in London.
>
> "That you have made so manie excellent observations of them I am most glad of. for you have gotten the starte of all in limitinge ther periods.
>
> "ther periods are verie merveilous especiallie that of Jovi proximus whos situation also is no lesse merveilous beinge not one diameter of Jupiter of from him.
>
> "of thes thinges and thos other mirabilia quæ indies invenis I longe to bee with you to discourse of them.
>
> "my wife is well. now you know all my comfortes.
>
> "I have lost my second boy also . . . .[b]"

In Galileo's first telescope the objects appeared "triplo viciniora noncuplo vero ma-"jora," (Sid. Nunc.); and he says, "tandem labori nullo nullisque sumptibus parcens "eo a me deventum est, ut organum mihi construxerim adeo excellens, ut res per ipsum "visæ millies fere majores appareant, ac plusquam in ter decupla ratione viciniores quam "si naturali tantum facultate spectentur:" but he adds, that it was necessary for observations like his own to have at least, "perspicillum exactissimum quod objecta pellucida, "distincta & nulla caligine obducta repræsentet, eademque ad minus secundum quater "centuplam rationem multiplicet; tunc enim illa bis decuplo viciniora commonstrabit." Harriot evidently uses the linear dimensions in marking his magnifying powers, and the "great glass" answers to what Galileo mentions as requisite. In 1611, on the 30th of January, he describes what he saw "with great instrument and small $\frac{20}{1}$ $\frac{10}{1}$." His first observation of the moon was made with $\frac{6}{1}$, but in the following April there is a memorandum on one of the papers containing an observation of the moon, that he then had "$\frac{10}{1}$ two, $\frac{15}{1}$ one, $\frac{32}{1}$ one, $\frac{11}{1}$ one." Indeed there seems to have been considerable difficulty in finding telescopes which would act well. Peyresc was not able to obtain one, with which he could see the satellites of Jupiter, before November 1610[c], although he had endeavoured to procure them from Holland and Italy, as well as from the persons who had already begun to manufacture them at Paris. He likewise applied to his brother at Paris, and "illum sollicitare, varieque urgere nunquam destitit, donec vitra "Telescopica confici curavit, misitque ad usque quadraginta[d]." Harriot, however, saw these satellites sooner, and it was not after all so much the "dulness" of his friend's sight, as the great strength of his own, which gave him the advantage. It will be seen that he observed the sun with his naked eye. For the satellites of Jupiter he frequently used only his less powerful telescope; and it is remarkable that Harriot himself frequently saw three, if not all four of the satellites, whilst his servant Christopher, whom he left in London with his instruments [e], was only once able to see two; although through the whole time he notes the weather, as affording "reasonable," "cleare," "fayre," or "starry nights."

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[b] This refers to the loss of his eldest son, which he had mentioned in a former letter, which will be given hereafter.

[c] Gassendi Opera, vol. V. p. 275.

[d] This agrees with Sir W. Lower's desire to have "more of all sorts of these cylinders." It is possible that in those early times the manufacturers were unable to adjust, or even to

After the detail of these particulars, there can no longer be any doubt of Harriot's having no claims to any [f] competition with Galileo for the discovery of Jupiter's satellites. The Baron de Zach, indeed, seemed latterly inclined to modify his original opinion of Harriot's having been "the first discoverer of these attendants of Jupiter;" but he still thought it "assez vraisemblable que Simon Mayer en Allemagne, Galilée en Italie, "et Harriot en Angleterre, avaient vu, en meme tems et chacun de son coté, les satellites "de Jupiter [g]." He argues, indeed [h], to Harriot's independence of Galileo, from his finding "this mathematician's name never quoted" in the papers. It is not easy to understand this, after what has now been laid before the reader. My principal document, indeed, has been found in the British Museum, and it is possible that the Baron's attention was more exclusively directed to the parts which probably he selected for his own immediate use, and which he afterwards sent to Oxford; but even in these it has been mentioned [i] that reference was made to the "Dissertatio cum sidereo nuncio," and if a more express notice of a name had been required, "Galilæi prosopopœia" might have been seen on the back of the paper which contains the rough notices of the first observations of the satellites.

It is most probable that Harriot lost no time in apprising his friend of what had come to his knowledge from abroad; and although Traventi is in Carmarthenshire, we cannot, after making every allowance for the difficulty in those times of distant communication, suppose that the news reached Harriot before the beginning of June. By this time, however, Jupiter was too near the sun for them to be able to look for his satellites [k]. This is the reason why Lower, with all his zeal and delight, still talks of the winter for his observations, and why Harriot himself did not succeed in making any before the middle of October 1610. All this is perfectly consistent; but it must not be concealed that the Baron de Zach says explicitly, "his first observation of those disco- "vered satellites I find to be of January 16, 1610." This is a positive fact, which is perfectly compatible with the planet's situation with regard to the sun; but the truth is that it is founded in mistake. The date, which caught the Baron's eye, belongs not to

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judge of their own work, and that it was necessary to purchase a number for the chance of having some good ones among them.

[e] See P. 22.

[f] See notes A, C.

[g] Corr. Astr. vol. VII. p. 126.

[h] See note C.

[i] P. 27.

[k] It appears from the Ephemerides of Maginus for 1610 that Jupiter in that year was in conjunction on the 13th of June, and in opposition on the 30th of December, O. S. Hence it rose and set not very long before the nebula in Cancer, which accounts for the constant reference to Præsepe made both by Harriot and Sir W. Lower.

an observation of Harriot, but to a table which he had calculated from Galileo's. To establish this point will require some calculation, and a close examination, the details of which would interrupt the present narrative: however, in explaining them, there will be an opportunity of enlarging on several other particulars, and the whole has therefore been subjoined at the end of the present tract[l].

From Sir William Lower's Letter it appears that Harriot began immediately to examine the motions of the satellites. The angular measures of them in their respective distances from Jupiter seem to have been one principal object to which he directed his observations, and the quantities which he deduced were "Semid. orb. 14', 9', 6', $3'\frac{1}{2}$." Harriot has in several instances set down distances of 12', 12' or 13', and once of 13', from the limb of the planet. This last estimate was very probably the foundation of his determination for the 4th satellite. 1611. Jan. 25. hor. 6. the distance is marked 12', or 13'; Jan. 26. hor. 10. 13', and Jan. 27. hor. 6. again 12' or 13'; so that if the measure on the 26th be increased by 1' for the semidiameter of Jupiter, there will be 14' for what might have been considered as the greatest distance from the centre. This is clearly too large. The quantity when Jupiter is in opposition is about 10', but great allowance must be made for the circumstances under which the observations were taken, and it must be remembered that, notwithstanding the clearer atmosphere of Italy in which Galileo observed, and the less indefinite manner of his estimating his angles, the distances in the Sidereus Nuncius are still too great[m]. Jupiter had past the opposition before he discovered the satellites in Jan. 1610, and yet he gives us distances of 10', 11', and on the 17th Feb. even of 12'. This is likewise the case with Simon Marius, who, in his Mundus Jovialis, makes the semidiameters of the orbits at a mean distance from the earth 13', 8', 5', and 3'; and there is a circumstance connected with these last quantities, which in the present inquiry may not be unworthy of notice. Marius says, "Deprehensum "est a me frequenti, diligenti & diurna observatione Jovem sua diametro in media a "terris elongatione unum minutum circiter subtendere." Now this excess producing a compensation of errors, his semiorbits will come out, in terms of the semidiameter of the planet, 26, 16, 10, and 6.—relative measures, which are not very different from those adopted by Newton in the beginning of the third book of the Principia[n]: but after all, his absolute quantities were in themselves erroneous, and nearly as much so as those of Harriot, who seems from this to have been mistaken rather in the fundamental estimate of 1', than in his application of it to the relative distances, which he observed.

Galileo watched the continued motions of the satellites, and in more than one instance marks the time when an emersion had just taken place. Harriot, indeed, Dec. 12, 1610, mentions one satellite, and an "other westerly, as I thought, 10" or 15" a ♃ "hardly seen:" but this is the only instance in which he notes any appearance even approaching to a conjunction; and he therefore had recourse, in a great measure, to the Sidereus Nuncius for determining the times of revolution. These he brought out 16.66, 7.315, 3.524 days, and 21.24 hours, or $16^d\,16^h$.; $7^d\,7.56^h$; $3^d\,12.56^h$. and

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[l] See note G.

[m] There are two which are set down as being 30'; but that of the 26th of Jan. ought clearly to be 30", and the other of the 26th of Feb. is possibly a press error of 10'.

[n] 26.63, 15.141, 9.494, 5.965.

21.24^h. It is well known that when Galileo published his Sidereus Nuncius he had not had time to ascertain these particulars, and Sir William Lower[o], in the beginning of March 1611, congratulates his friend Harriot on having "gotten the starte of all in "limitinge ther periods." There is likewise another accidental circumstance which proves the originality, as well as the early date of his calculations. The three first periods are possibly as near the truth as it might at first have been expected that he should have arrived; but by a mistake in the number of revolutions, which he assumed in a given interval for the first satellite, he brought out its time only half of the truth. Now Simon Marius, in his Mundus Jovialis, makes the times $16^d\,18^h\,9'\,15''$; $7^d\,3^h\,56'\,34''$; $3^d\,13^h$. $18'$, and $1^d\,18^h\,28'\,30''$; and it is therefore clear that Harriot must have determined the quantities for himself previous to the publication of this nearer approximation to the truth. It is not improbable, also, that if he had not ascertained, he had at least suspected his error for the 1st sat. at an earlier time; for in a paper entitled "Jovi—primi Motus" he had originally written his quantities as derived from a revolution completed in 21^h. 24, but he has added another column, in which he has doubled them all, and written at the bottom "ut periodus 42^h. 48." This second column is evidently written subsequently to the rest of the paper, and possibly before 1614[p], when Marius's book came out; because there is no erasure, or any thing to mark that the erroneous numbers are not to be used. The error is the more difficult to understand from Harriot's having well studied the great work of Kepler, in which he had shewn that the squares of the periodic times were as the cubes of the major axes for the primary planets. There was such an obvious analogy likewise, that Galileo, when he was satisfied of the existence of the satellites, expresses it by saying, "in cœlis adesse stellas vagantes circa Jovem, instar Veneris atque Mercurii circa "solem." It may seem wonderful therefore to us that Harriot, if he entertained any doubts between the two periods, did not apply Kepler's law to them as a criterion. That a fair judgment however should be formed on a point like this, we should endeavour to lay aside all considerations but those which were then in existence. The law in question was only ascertained in Harriot's time as a fact, which had been collected from the motions of the planetary bodies; no reason for it was then known, and the astronomer might not be prepared, *à priori*, to argue on the admission of it in a system of secondary planets. The most philosophical method was to ascertain all the facts first from observation, and to inquire afterwards whether the law extended to the new case which then presented itself.

III. Next to Harriot's observations of Jupiter's Satellites, those to which the public attention has been principally called were made by him of the spots on the Sun. They are 200 in number, and are contained in seventy-three half sheets of foolscap, besides three smaller pieces of paper. A facsimile of the first of these half sheets will be found in Plate III. Sup., and the contents of it are as follow:

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[o] See P. 28.

[p] Lalande (Bibliogr. Astron. p. 158.) mentions "Simon Mayer oder Marius Frankischer "kalender, oder Practica;" which was published in 1612. He says that the author had already calculated the revolution of the "deux "extremes" of Jupiter's Satellites. By this description the third and fourth are probably meant: there is no evidence of Harriot's having seen the book, but he certainly was acquainted with the Mundus Jovialis, for there are calculations made with the tables contained in that work.

> " 1610 Syon December 8^{d} mane ♄. The altitude of sonne being 7 or 8 degrees. It being a frost & a mist I saw the sonne in this manner.
>
> " Instrument $\frac{10}{1}$ B. I saw it twise or thrise. once with the right ey & other time with the left. In the space of a minute time. after the sonne was to clear.
>
> " $\frac{1610}{1611}$ Syon January 19 ♄ a notable mist. I observed diligently at sondry times when it was fit. I saw nothing but the cleare sonne both with right and left ey.
>
> " Syon 1611 Decemb. 1 ⊙ mane ho. 10 0 per horologium solare
>
> " I saw three blacke spots in such order as is here expressed as nere as I could judge. observed by $\frac{10}{1}$.
>
> " S^{r} W. Lower with $\overline{\text{Chr}}$ also saw the same. at sundry times all three seen & observed at once for halfe an houre space. at which time and all the morning before it was misty.
>
> " The greatest was that which was most oriental appearing somewhat ragged & was of apparent angle about 2′, the other two were nere of one bignes : & of 1′ magnitude or there aboutes."

The papers are in good preservation, the writing is sufficiently clear, and the drawings are well defined. The Baron de Zach says, that he " compared the corresponding ones " with these observed by Galileo, between which he found an exact agreement." This however is more than can be justly asserted of them; it should be rather said that there is that degree of general resemblance, which marks the identity of the objects that were observed. Harriot's drawings were made with a pen and common ink, which did not admit of shading. Indeed he repeatedly mentions that spots, and parts of spots were " dim," or not so dark as the rest; and he endeavours to distinguish this in his designs, but in no way that approaches to the real appearances which present themselves on the sun. The shapes likewise, when he attempts to imitate them, are very roughly delineated, nor can we always distinguish the narrower figure[q] which the spots assume, as they are nearer to the sun's limb. His drawings are certainly much superior to the coarse wooden cuts inserted in the collection of Galileo's works; but in the original edition of the Istoria e dimostrazioni intorno alle Macchie solari (Roma 1613) the phenomena are represented on copper plates, with great attention to their respective forms, and to the penumbra by which they may have been accompanied. To enable any one to judge of the fact for himself, a copy of Harriot's observation, June 26, 1612, is given in Plate IV. Sup. This particular instance was fixed upon, because it seems to have been especially noticed by the Baron de Zach, and the reference to Galileo on the side of it is in his handwriting. The representation for the 6th July (N. S.), will be found in p. 91. of the Istoria. The Baron has likewise noted p. 90, because this observation having been made about 6 o'clock in the morning of the 26th, answers to an earlier hour than that for which we have the drawing of Galileo for the 6th of July. In fact, Harriot's must be considered as a representation of the intermediate state between those in p. 90, and 91. He has left us one for June 25 which gives very nearly the position of the spots as they appear in p. 90; but it would not be a fair specimen, because it was made in a hurry, as will be seen from the memorandum on the drawing for the 26th. It appeared better therefore to add the observation of June 11, which seems to have been more nearly contemporaneous with Galileo's of the 21st[r]. (p. 77.)

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[q] He had not however failed to notice the variation; 1611, Dec. 15. " they generally " seeme bigger, being nere the middle then the " sides."

Independent of the materials with which Harriot made his drawing, there was an essential source of inferiority from his method of observation. Scheiner, in his first letter to Velserus, says, "Sol ubicunque apertus per tubum, præter convexum & concavum vitrum, vitro insuper utrimque plano cœruleo aut viridi debite crasso munitum, ea ex parte qua admovetur oculus, indemnes adversos servat oculos vel in ipsa meridie[s];" and in the tract, "de Maculis solaribus," which he published eight months afterwards, he mentions[t] the more easy method of observing the spots by receiving the sun's light through a small aperture, "perpendiculariter in chartam mundam." Galileo, at the suggestion of his pupil Benedetto Castelli[u], admitted the light not merely through the opening, but through a telescope, which was directed to the sun. By this means he was able to distinguish even the smaller spots, on a paper which was held perpendicular to the axis of the cone of rays. It appears from the Rosa Ursina, that Scheiner also adopted this improvement, but Harriot did not take advantage of any such assistance. There is not the slightest allusion to any thing like the camera obscura, and although he speaks in one place (1611, Dec. 13.) of observing "thorough my coloured glasses," he appears afterwards to have constantly laid aside the use of them.

Greaves says[x], that in measuring the diameter of the sun he hurt his sight, "insomuch that for some days after, to that eye, with which I observed, there appeared, as it were, a company of crows flying together in the air at a good distance. At the first I did verily believe I saw a company of crows flying in the air." Lalande[y] quotes from Scheiner, that the first inventor of telescopes lost his life from an inflammation of the eye, brought on by looking at the sun; he then mentions the blindness which came upon Galileo and Cassini in their old age, and warns astronomers against the fatal negligence of necessary precautions in observing. But it must have been something very different from negligence which could have operated so widely, and upon such men as we have just mentioned. Hooke says, "as to the coloured glasses I cannot at all approve of them, because they tinge the rays into the same colour, and consequently take off the truth of the appearance as to colour; besides, it superinduces a haziness and dimness upon the figure, so that it does not appear sharp and distinct[z]." The first of these objections is not of any very great importance, but the second suggests a reason why Harriot and others may have abandoned the use of darkened glasses. If the texture of the glass were not uniform, if the colouring matter were not clearly and equally distributed through it, if the surfaces were not worked to good planes, or if the opposite sides were not parallel, the image seen through the glass would not be accurately represented. In the early state of the arts these evils were very likely to occur, and it can be easily understood that such instruments might be well thrown aside by those, whose ardour of observation overpowered the consideration of personal ease and safety. It might be from reasons of this kind that Harriot was willing to observe with his naked eye, and for that purpose took the opportunity of misty weather, or thin clouds, which might diminish the intensity of the sun's light. He preferred the latter; for, he says, (1611, Dec. 14,) "The sonne was somewhat to cleare. There being no "cloudes but only thick ayer. And then the spots seme lesse and are not also so well "seen as otherwise." In this manner the very last observations, which are recorded (for the 14th and 18th of January, 1613,) were made when there were "thick ayer & thin "cloudes," "a thin mist and thin cloudes." But he sometimes ventured to trust the strength of his sight without any of these protections. He says, (Dec. 21, 1611,) "afternoon at $2\frac{1}{4}$ when the ⊙ had no cloudes nor mist, I saw the most oriental fayrest "& the 3 orientall of the 4 in a rank;" and, (Feb. 17, 1612,) "all the sky being cleare, "and the sonne, I saw the great spot with $\frac{10}{1}$ and $\frac{20}{1}$, but no more[a], my sight was after "dim for an houre."

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[r] The memoranda annexed to the observations may not be easily legible to every one; they are as follow:

"London ♀ June 26 ho. 5½.6. mane. Thicke ayer & thin clouds. 19 spots great & small. places better conjectured than the day before having had more time. the 4 small spots not so blacke as the rest.

"♃ June 11 ♃. ho. 3½ p. m. Thorogh thin cloudes 11 spots. now 5 in the cluster fayre. 6 more, of which the 2 southermost placed by conjecture. having not time inough to examine there places but not much amisse."

[s] p. 4.

[t] p. 40.

[u] Opere (Padoue, 1744.) vol. II. p. 109.

[x] Misc. Works, vol. II. p. 508.

[y] Astron. §. 2474.

[z] Description of Helioscopes, p. 3.

The two magnifying powers which are here noted, are the same which were used for the satellites of Jupiter, but others are mentioned in the observations of the solar spots; and he says, (Dec. 13, 1611,) "it seemed to me that a greate glasse would make them "appeare divided and more dissevered:" accordingly he used not only $\frac{8}{1}$, and[b] two $\frac{10}{1}$ as well as $\frac{20}{1}$, but $\frac{15}{1}$, $\frac{30}{1}$, and $\frac{50}{1}$[c]. But the advantages which might have been derived from powerful instruments, were more than counterbalanced by the manner of using them. When the difficulties are considered, which must necessarily occur in hastily looking at the sun, and then, with the eyes dazzled by its overpowering light, in laying down on paper the figure and position of the spots which may have been seen, it is no small praise for Harriot to have drawn them near the truth; and under such circumstances it is not wonderful that his designs should be very inferior to Galileo's. The sun's strongest light, and the clearest air, were best for shewing the picture in a darkened room; but on many occasions the smaller spots must have escaped observation when obscured by the very state of the atmosphere, which was required for a direct view of the solar disc. Thus on the 5th of Dec. 1611, there is a memorandum that "$\overline{\text{Chr}}$ "saw one more orientall sometimes as he sayd and is noted;" and on the 11th of the same month, "I thought that sometimes I saw the two which were seen on Munday "nere the middle but obscurely and for no time therefore could not note there posi- "tion." A deliberate examination with the naked eye must often have been impossible:

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[a] He had seen eight spots in the morning.

[b] This will account for his distinguishing one of them as $\frac{10}{1}$ B. (P. 32 and 35.)

[c] $\frac{30}{1}$ is spoken of on Jan. 20, and $\frac{50}{1}$ on Jan. 31, 1612. But there is no notice of his having used these more powerful instruments for the important purpose of observing Jupiter's satellites, although he continued his attention of them till the 26th of February of the same year.

thus, (Dec. 15, 1611,) "I suspected some ragges on the north side of the spot. which in "some more convenient light an other time may be discovered. The position as I con-"jectured having but little time is as in the 2 figure." But when every thing was most favourable, Harriot had no certain means of determining either the size or situation of what he observed: the field even of the smaller telescope, which he commonly used, did not take in the whole of the sun's disc, for he says, (Jan. 11, 1612,) "one of the spots "was in the verticall the semidiameter of my instrument B $\frac{1}{1}^{\circ}$ from the upper edge and "the whole diameter from the lower." The spot itself is placed accordingly on the perpendicular diameter of the circular figure which is annexed; but we find a notice in the margin, "B 30′," which, if it is meant to describe the angular value of the field of view, will be another instance, in addition to those already adduced, (P. 24,) of the little precision with which these estimates were made.

There is much obscurity with respect to the precise time when the first observations were made with telescopes of the spots on the sun. The earliest account which is known to have been printed of them is in the Tract of Jo. Fabricius Phrygius, entitled, "De "Maculis in sole observatis," which was published at Witteberg in 1611, and the dedication of which, "Ennoni totius orientalis Phrysiæ comiti," is dated "Id. Junii" of the same year. In the Memoirs of the Royal Academy of Sciences of Paris for 1778 [d], Lalande gives the particulars of this little work, and an account of what it contains on the present subject. This clearly exhibits the most important facts, and saves the tædium of reading through much which is not possessed of equal interest; but it conveys no idea of the honesty with which the original is drawn up. Fabricius betrays no eagerness for putting himself ostentatiously forward, but rather deprecates the severity of any remarks to which he may be exposed. He is careful not to overstate any thing: in speaking of Galileo's discovery of Jupiter's satellites, "Quos non tantum multis visos, "sed etiam observatos aliquoties constat," he says, "verum subtilitate hic opus est sum-"ma, ne et oculus atque instrumentum nobis imponant, alioquin proclivis hic lapsus "est et facilis eorum persuasio, ob infinitas affixorum siderum luculas in planetarum "confiniis constipatas. Equidem si mihi ipsi fido, eos mihi visos non ausim simpliciter "negare. Verum quia instrumenti mei subtilitas non omnino attingebat tenuitatem "illarum stellarum, malo ista observationis fide carere, quam obscuræ licet aliquoties "repetitæ animadversionis testimonio temere famam augere novæ observationis." In the same spirit he speaks of the motion of the spots round the body of the sun [e]: "Id "vero non ex unica saltem revolutione persuadere mihi nec potui nec volui, ne me at-

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[d] P. 393.

[e] The method of observing the sun without exposing his sight to the perils of direct vision, occurred immediately to Fabricius; "Noluimus deinde oculos fatigare amplius isto "onere, sed cepimus consilium, ut si quo com-"pendio eos in posterum levare possemus, id "minime prætermitterimus. Cogitavimus igi-"tur de radiis solis per angustum foramen "intromittendis, & in obscura, clausis fenes-"tris camera observandis. Notum enim est "ex Opticis, quod omnia, quæ foris sunt & "aguntur, in tenebroso cubiculo possunt re-"præsentari, aperto solum angusto quodam "foramine, per quod species rerum ipsi fora-"mini objectarum, illabantur, ut pingant parie-"tem in cubiculo oppositum, sed omnia in-"verso situ. Hoc igitur artificio consulentes "rei difficultati, solis imaginem in Cameram "obscuram intromisimus atque illabentem in "convenienti a foramine distantia papyro ex-"cepimus."

" que alios deciperem, sed ex aliquot sequentibus, quas ab anni hujus initio ad hoc usque " tempus non tantum ego solus notavi sed alii etiam mecum ad conciliandum huic rei " fidem et persuadendum illis, qui forte hoc observationis negotium sibi molestum aut " tædiosum existimaturi sunt." This is, unfortunately, the only passage from which we can collect any thing about the time of his discovery. Lalande[e] considers that it carries us back " à peu près au commencement de 1611 ;" but it seems to go somewhat further. From the cautious, unassuming character of Fabricius, it is most probable that he observed long enough to be convinced of the general truth, before he began the series of observations in which he associated others with himself; and as it would require nearly four weeks for the spots to return even once to the same part of the sun's disc, on which they had been originally seen, there will be no improbability in supposing that he had discovered them in 1610. The general style of his account gives every reason to believe, that he made no unnecessary delay in admitting his friends to a share in the pursuit of this interesting subject; but, on the other hand, it must be recollected that it appears from Harriot's account that no spots vere visible on the sun in the middle of January 1611.

Scheiner claims to have seen the spots, " anno millesimo sexcentesimo undecimo mense " Martio[f]," but he probably made no observations on them before the following October: by his own account he was therefore later than Fabricius, even if he could have supported his cause in opposition to Galileo. The great philosopher of Florence did not publish on the subject before 1613, but his " Istoria è dimostrazioni intorno alle macchie " Solare," which came out in that year, contains the letter written to Velserus in May 1612[g], which speaks of his having then observed them for eighteen months. I believe myself that he had seen them at a still earlier period, but it is unnecessary to enter further into the inquiry on the present occasion[h]. The Baron fixes the epoch of Galileo's discovery to be in the month of November 1610, which is evidently anterior to Harriot's first observation; but, to avoid the obvious inference from this concession, he argues against the probability of Harriot's having any notice of the observations which had been made in Italy. Had he confined himself to the point in question, there could have been no doubt of the fact. The controversy between Scheiner and Galileo could never have existed if the discovery of the solar spots had been generally known at so early a period even on the continent; and *à fortiori* we cannot suppose that the news had reached England. This is conclusive, as far as it goes, in Harriot's favour; but the Baron seems to wish for more, and in his anxiety to get rid of Galileo, endeavours here to shew that his discoveries were wholly unknown to the English mathematician. This is sufficiently improbable in itself, and after all that has been already detailed on the subject, there can be no occasion for repeating the refutation of it; but the argument introduced on the other hand against the originality of Harriot must not pass unnoticed. The Baron says, "it rather seems that Harriot had taken the hint from Josephus à Costa's Natural "and Moral History of the West Indies; in which he relates, that in Peru there are "spots to be seen in the sun, which are not to be seen in Europe." A specific reference is given on this head to b. I. ch. 2. which shortens the search, and enables us immediately to see that the whole is founded in mistake. A Costa certainly speaks of spots, but not on the disc of the sun [i]: he distinctly describes them as seen in the heavens; and he evidently refers only to those dark parts which have been so often noticed among the constellations of the southern hemisphere [k].

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[e] Astronomie, §. 3225.

[f] Rosa Ursina, p. 28. A little higher in the same page he had said, " maculas mense Martio " anni 1610 inspexit et ostendit;" but this must have been a misprint, or must imply that Scheiner intended to designate the year which ended on the 25th of March.

[g] It may not be generally known that the original of this remarkable letter is in the British Museum.

[h] This is a question well worthy the attention of the author of the life of Galileo. There is no one in our own country better able to discuss it: we may hope to see his work in a form more suited to its merits, and that an enlarged edition will enable us to profit from his examination of this interesting question.

To return to Harriot. His observations of the solar spots are great curiosities: like those of Jupiter's satellites they are the earliest original notices which are known to exist, and there is reason to believe that the first of them was made from his own spontaneous curiosity. Absolute priority of time is not necessary to the proof of originality: the observation however cannot, with any degree of justice, be rated so highly as the Baron would place it. There is a great fallacy in saying that "they run from Dec. 8, 1610, till January "18, 1613." These certainly are the first and last dates, but the series really begins only at Dec. 1, 1611 [l]. Harriot's entry of Dec. 8, 1610, is evidently worded in a manner which impresses us with the idea of his having unexpectedly seen what he could hardly persuade himself to be real. He looked at it repeatedly with each eye, till the increasing brightness of the sun prevented him from continuing his observation. Had he soon been again able to observe the sun, there can be little doubt that he would have ascertained the existence of the phenomenon; but it appears that he did not repeat his observation till the 19th of January, 1611, and then, most unfortunately, the solar spots had wholly disappeared. The consequence was, that he did not look again after them till the following December; before which time their existence had been fully ascertained by others, and the account of what had been seen on the continent must have reached England. Accordingly, he then designates the phenomena by the specific name of spots, whereas, in the first instance, (Dec. 8, 1610,) he appears to have had no appropriate terms in which he could describe them, and merely refers to his drawing for the appearance, which he was desirous of recording. His earliest observation, indeed, was no more than a transient view of a phenomenon, which had been seen before his time, even with the naked eye; and although I am not inclined to speak of Harriot as Delambre speaks of Scheiner [m], I must say that it is a misapplication of terms to call such an observation a discovery. The Georgium Sidus was seen by Flamsteed, Mayer, Le Monnier, and Bradley, but no one would claim for either of them the discovery of the planet, in preference to sir William Herschel.

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[i] See the whole passage at note F.

[k] Lalande, Astronomie, §. 840. There is also Dacosta, disputatio philosophica de Maculis Solis. It is not impossible that some indistinct traces of this title may have been floating in the Baron's mind, and have contributed to lead him into the mistake. But the tract was not published by the author, Alvarus Telles Dacosta, till 1734.

[l] Harriot marks this distinction himself. He numbered his observations from 1 to 199, but he began with that of Dec. 1, 1611. There is no number affixed to that of Dec. 8, 1610, and this is the reason why I have called the sum of the whole 200 instead of 199, which the Baron mentions.

[m] S'il avait une lunette, il a pu voir les taches; il n'y a pas grand mérite à cela. Hist. de l'Astron. Mod. vol. I. p. 631.

Against the observation of Dec. 1, 1611, there is the mark [☌ ☉ ♀], and there can be no doubt that it refers to the conjunction predicted by Maginus, the notice of which is inserted with the same symbols against the same day in his Ephemerides. It appears to have excited considerable attention. Venus at the time was in the superior part of her orbit, and her motion was therefore direct. No transit could have taken place; but, in spite of all which telescopes had laid open, there were some who still clung to the possibility of the Ptolemaic system, and they argued very justly, that the doctrine of Venus being always below the sun would be established, if, under such circumstances, it was seen upon its disc. Scheiner, therefore, discusses the event very particularly. In his second letter to Velserus he points out that Venus was moving in a direction contrary to that in which the spots cross the disc: the one therefore could not be mistaken for the other. As Venus would advance with only the excess of her geocentric velocity above that of the sun, the phenomenon, if it took place, would extend over a considerable time; according to Scheiner it was to commence at 11 on the night of Sunday Dec. 11, (N. S.) and to last for at least forty hours. According to these numbers the termination would not take place till 3 in the afternoon of Tuesday the 13th. Monday the 12th was entirely cloudy, but, on the contrary, Tuesday was clear and fair, from the earliest dawn till late in the afternoon: Scheiner therefore, and others with him, took this opportunity of carefully observing the sun from its first rising. Of course they saw nothing, and he therefore concludes that "etsi careremus omnibus aliis argumentis, hoc uno "evinceretur solem a Venere ambiri." He afterwards entered more minutely into the question, in his "Disquisitio de Maculis Solaribus," but it is unnecessary at present to follow him any further.

Though the precise nature of the connection is not obvious, some evidently existed between the expectation of this conjunction and the renewal of Harriot's attention to the spots on the sun. His own labours and his acquaintance with the works of Copernicus, Kepler, and Galileo, make it highly improbable that he could have had any doubts with respect to the true system of the world; but others about him may not have been equally satisfied on this point, and, on their account, he may have determined to mark the result of the expected conjunction. To examine the spots which might be on the sun at the time was an useful preliminary, for which he may have taken advantage of the mist that presented itself on the Sunday morning. As there is no delineation for the 2nd of December, the weather on that day was probably unfavourable for him, as it was for Scheiner; but in the morning of Tuesday the 3rd, he observed the sun again with sir William Lower and Christopher, at 8½ and 8¾. There is no notice taken of the planets not being seen, but the recognition of the spots which had "appeared "December 1," not improbably fixed Harriot's attention, and induced him to pursue the observations. It is no proof of the truth of this conjecture, but it is perfectly in accordance with it, that the observations of Dec. 1 and 3 are written in a larger hand and manner than will quite assimilate with the form which he afterwards almost immediately adopted for his entries, and to which he uniformly adhered till the 7th of June 1612.

That the spots might be produced by some opaque bodies revolving at short distances about the sun, was an hypothesis which occurred to some of the first observers of the fact; but if it did pass through Harriot's mind, it probably did not make any permanent impression on it. The supposition was at variance with the appearances which the spots exhibited, and with the length of time which they all uniformly took up in making their revolutions round the sun. Now we find that Harriot had attended to this period, although our expectations would be much disappointed if we looked for the strict "determination of the sun's revolution round its axis." There are two half sheets containing calculations for this purpose[n], which have been noted in red ink by the baron, so that no mistake can be made with respect to the part of the manuscripts to which he intended to refer. The first of these papers is occupied by two tables, to shew the number of days which may have elapsed at the end of every month, 1st, from the beginning of January to the end of the year, and 2nd, from the beginning of Dec. 1611 to the end of Jan. 1613. The following numbers are all that are material in the second paper, the rest being nothing but the arithmetical processes by which the results are worked out.

$$\left.\begin{array}{l}\text{1610 Dec. 8}\\ \text{1611 Nov. 28}\end{array}\right\}\ldots 355\,d=\genfrac{}{}{0pt}{}{27}{13}+\frac{4}{13}$$

$$\left.\begin{array}{r}\text{Dec. 3}\\ \text{Dec. 22}\end{array}\right\}\ldots 385\,d=\genfrac{}{}{0pt}{}{27}{14}+\frac{7}{14}$$

$$\begin{array}{l}\text{1610 Dec. 8}\\ \text{1612 Dec. 17}\end{array}$$

$$\left.\begin{array}{l}\text{Dec. 21.20}\\ \text{May 30}\end{array}\right\}162\,d=\genfrac{}{}{0pt}{}{27}{6}$$

The meaning of this is very obvious: in the first comparison he deduced from the supposition of 355 days containing 13 synodic revolutions, that each of them was in length 27 days and $\frac{4}{13}$; the others give analogous conclusions, but there is considerable difficulty in understanding the grounds on which the comparisons were made. Harriot certainly observed the solar spots on the 8th of December, 1610, but he has left us no observation of the 28th of November, 1611. Scheiner, indeed, in his "Epistolæ ad Marcum Velse-"rum," gives some drawings made on this latter day, (Dec. 8, N. S.) but they bear no resemblance to Harriot's first design, which could lead to the supposition of the spots being identical at the two epochs. Harriot saw the spots on the 3rd of December, 1611, but he has left us no observation of the 22nd of December in the following year[o]. There is some partial resemblance in his drawings for Dec. 8, 1610, and Dec. 17, 1612, but he has deduced nothing from it,—this interval, if it is supposed to contain 27 revolutions, would give 27 days and $\frac{10}{27}$ for each. His observation of Dec. 21, 1611, compared with those of May 29 and 31, (there is none of May 30,) 1612, exhibits nothing from which any deduction can be made. There was in fact no possibility of diminishing the error of the period by the comparison of very distant observations: the advantage of such a process is familiar to every astronomer, but in the present instance the use of it was precluded by the fleeting nature of the phenomenon. Harriot's acute mind must have perceived the difficulty, especially when he had the experience of so long a series of observations; and it is impossible to consider these calculations as any thing like a precise determination of the period of the sun's rotation.

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[n] On the back of the paper containing observations from Feb. 21 to April 7, 1612, there are some numbers which seem connected with the time of the sun's revolution, but it is not easy to understand how they are supposed to contribute to the determination of it.

[o] He saw the spots on the 23rd, and there is some little resemblance in his design for that day to what he had drawn for Dec. 3, 1611.

Such are the principal astronomical observations which really exist; but there are others to which the Baron de Zach specifically alludes, and which must not therefore pass unnoticed.

Annexed to the observation of Jupiter's satellites for May 24, 1611, there is a note that "♂ by our instrument was about 31′ or 32′ g. from ♃[p]," and "the night before "about 30′ or 31′." It had been previously mentioned that the observation of the satellites was made at 9 or 9¼, "the time being twilight, and ♃ low;" there is also a rough drawing copied in fig. 2, plate IV. Sup. but there is nothing to mark more precisely the time when the two planets were observed; and this memorandum is the only observation of Mars which I have been able to find that Harriot has recorded of his own. He wrote much indeed upon the subject, for he studied (as I have mentioned before) the treatise de Stella Martis with the greatest attention; and among his papers on this subject there are many observations mentioned of the planet; but so far from any of them having been made by Harriot, he has marked them all with the very pages of Kepler's book, from which they have been taken.

"Of solstices," equinoxes, the length of the year, &c. there are many calculations from the data to be found in the writings of earlier astronomers; but I have not been able to discover a single observation connected with any of them, which was made by Harriot himself. Wood, in his Athenæ Oxonienses[q], mentions a work of his, entitled, "Ephemeris Chyrometrica, MS. in the library at Sion college, Lond." Nothing of the kind appears in Reading's catalogue of that collection, and it is probably lost; but among Harriot's papers there are some drawings of the hand, with mnemonic lines, which, being referred to the several parts and joints of the fingers, give a technical method of finding the dominical letters, the day of the week for any particular time, &c. &c. Wood's orthography requires some emendation, but the correction is so obvious, that with this explanation there can be very little doubt of the probable contents of the volume to which he alludes. There is a contemporary publication on somewhat the same plan, entitled, Pompeii Limpii Dactylismus ecclesiasticus. Ven. 1613.

"Of refractions" there is nothing of Harriot's in the Petworth papers. There are three tables indeed, but they are copied from pp. 79, 124, 280, of the Progymnasmata of Tycho Brahe: and there are two short passages, which occur besides on this subject, but they are expressly stated to have been taken from Tycho. The work is not mentioned on them, but the pages of it are noted, and the extracts were certainly made from the volume, entitled, "De Mundi ætherei recentioribus phænomenis liber secundus." There is much on refraction in the British Museum, but it is generally confined to those cases in which the rays of light pass through a transparent medium of a regular shape: of astronomical refractions I was able to find very little, and in that little nothing of any importance. From these papers it is clear that Harriot was acquainted with the law which is deduced from the sines of the angles of incidence and refraction, but I did not mark any facts which could shew that he had used it before the time when it was discovered by Willebrord Snell[r].

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[p] In P. 23 there is mention of the near approach of Venus and Saturn. These are the only observations of the kind which have been remarked.

[q] Vol. II. p. 301.

There is a book, on which is written, "The Doctrine of Nautical Triangles," and it contains one page "for the variation of the compasse by the east & west amplitude of "the sunne's risinge and settinge," but the directions do not contain, even in the form of an example, any quantity which can be supposed to be an observation which Harriot had made for himself of "the declination of the needle." In the same manner the papers in the British Museum, though several refer to the subject of magnetism, contained no observations, which I could find, that Harriot had himself made of the position of the needle.

I am fully aware of the difficulty in establishing a negative, but I trust that if a future examination shall detect any thing which I have inadvertently overlooked, the mistake will not be found in the papers, which, by lord Egremont's indulgence, I have had the opportunity of examining at leisure. These, indeed, I wish particularly to rest upon, and any one may now judge of the little call which there was for printing them. The whole deserves to be preserved with every care; but their value centres in themselves: it does not depend so much upon the knowledge which can now be acquired from them, as on the great interest of such early records. If the Baron de Zach had, in the first instance, drawn up a fair report, and had had a few of the drawings copied as specimens, all would have been done which could have been of any use: whatever might be thought of omitting the configurations of Jupiter's satellites, the delineations of the spots on the sun form the essential descriptions of the observations, and to have had them all engraved would have been an enormous waste of property, especially at a time when facsimiles could only have been taken on copper. It must likewise be kept in mind, that the Baron had published the observations of the comets of 1607 and 1618 before he sent the papers in 1794 to Oxford. It is now discovered that he had not printed them correctly; but it was highly probable that this circumstance was not even suspected at the time, especially as they appeared in a German publication, which was not likely to be so well known in the latter part of the last century as it would have been at present.

It is much to be regretted that these facts were not more generally understood; but those who had taken an inaccurate but popular view of the subject, continued long to impress the public with very erroneous and exaggerated opinions. No one, possibly, contributed more to this in our own country than Dr. Hutton. The paper which he reprinted in his Dictionary under the article Harriot, was followed up by what the Baron de Zach published in his Monatliche Correspondentz[s] for July 1803, which spread the story on the continent. Dr. Hutton's second edition of his Dictionary in 1815, contained many additions and corrections; but the account of Harriot was left untouched, although he might have easily ascertained that the proposed publication was then given up, and that the papers had been restored more than fifteen years before to the earl of Egremont. Professor Playfair recalled public attention to them in the Edinburgh Review[t], and as he wrote under the common misconception, Dr. Robertson endeavoured to correct it by communicating his own Reports to sir D. Brewster, who published them in the Ed. Phil. Journal for April 1822.[u] The evil, however, had taken too deep a root to be removed by this plain statement, which was drawn up for a special purpose. It only discussed what was really to be found in Harriot's papers, and was therefore thought by many to have omitted much which they imagined to be contained in them: and the Baron de Zach drew up a reply[x], which must not be left unnoticed on the present occasion. It would be to very little purpose if time were consumed in a critical examination of the whole of his remarks; but there are some which require explanation, and which are principally founded on the following extremely curious and important letter.

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[r] It is not conclusive, but some presumption may be formed on this point from Harriot's having been certainly familiar with the works of Snell. There is a number of papers with the calculations which he made when studying the Eratosthenes Batavus; and there is a bookseller's bill, in which charges are made for a copy of it, and also of the Observationes Hassiacæ.

" Fragment d'une lettre de Henri Percy comte de Northumberland, prisonnier d'état, détenu dans la tour de Londres, à Thomas Harriot, Sion Collège, à Londres.

" I have received the perspective cylinder that you promised me and am sorrie, that my man gave you not more warning, that I might have had also the 2 or 3 more that you mentioned to chuse for me. Hence forward he shall have order to attend you better and to defray the charge of this an others, for he confesseth to me, that he forgot to pay the worke man.

" According as you wished I have observed the moone in all his changes. In the new I discover manifestlie the earthshine, a little before the dichotomie that spot which represents unto me the man in the moone (but without a head) is first to be seene. A little after neare the brimme of the gibbous parts towards the upper corner appaere luminous parts like starres, much brighter then the rest and the whole brimme along, lookes like unto the description of coasts, in the dutch bookes of voyages. In the full she appeares like a tarte that my cooke made me the last weeke. Here a vaine of bright stuffe, and there of darke, and so confusedlie al over. I must confesse I can see none of this without my cylinder. Yet an ingenious younge man that accompanies me here often, and loves you, and these studies much, sees manie of these things even without the helpe of the instrument, but with it sees them most planielie; I mean the younge Mr. Protheröe[y].

" Kepler I read diligentlie, but therin I find what is to be so far from you. For as himselfe he hath almost put me out of my wits, his aequants, his sections of excentricities, librations in the diameters of Epicycles, revolutions in Ellipses, have so throughlie seased upon my imagination as I doe not onlie ever dreame of them, but oftentimes awake lose myselfe, and power of thinkinge with to much wantinge to it, not of his causes for I cannot phansie those magnetical natures, but aboute his theorie which me thinks (although I cannot yet overmaster manie of his particulars) he establisheth soundlie and as you say overthrowes the circular astronomie. Doe you not here starthe, to see every day some of your inventions taken from you; for I remember long since you told me as much, that the motions of the planets were not perfect circles, So you taught me the curious way to observe weight in Water, and within a while after Ghetaldi comes out with it in print. A little before Vieta prevented you of the gharland for the greate invention of algebra. Al these were your deues and manie others that I could mention; and yet to great reservednesse hath robd you of those glories. But although the inventions be greate, the first and last I meane, yet when I survey your storehouse, I see they are the smallest things, and such as in comparison of manie others are of small or no value. Onlie let this remember you, that it is possible by to much procrastination to be prevented in the honor of some of your rarest inventions and speculations. Let your countrie & frinds injoye the comforts they would have in the true and greate honor you would purchase your selfe by publishing some of your choise workes. But you know best what you have to doe. Onlie I, because I wish you all good, wish this, and sometimes the more longhinglie, because in one of your letters you gave me some kind of hope thereof.

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[s] Vol. VIII. p. 30.

[t] Vol. XXVI. p. 36.

[u] Vol. VI. p. 314. See note H.

[x] Correspondence Astron. vol. VII. p. 105.

[y] The Protheroes are an ancient family, deriving their name in Welch from Ap Rhydderch. Among the papers in the British Museum there is " A note of the papers and bookes " in Mr. Harriot's trunke delivered to Mr. Torporley," which is marked as " copyed from " Mr. Protheroe."

“ But againe to Kepler I have read him twice over cursoridlie. I read him now with calculation. Sometimes I find a difference of minutes, sometimes false prints, and sometimes an other confusion in his accounts, these difficulties are so manie, and often as here againe I want your conference, for I know an hower with you would advance my studies more than an yeare heare, to give you a tast of some of these difficulties that you may judge of my capacitie. I will send you onlie this one . . .

[“ C'est ici que le comte expose ses calculs au long sur le ‘ locum Martis’ du chapitre XXVI page 137 de l'astronomia nova . . . de motibus stellæ Martis etc de Kepler.”]

. . . . for his theorie I am much in love with these particulars ;

“ 1°. His permutation of the medial to the apparent motions, for it is more rational that all dimensions as of eccentricities, apogaeies etc —— should depend rather of the habitude of the sun, than to the imaginarie circle of orbis annuus.

“ 2°. His elliptical iter planetarum for me thinkes it shewes a way to the solving of the unknown walkes of comets. for as his ellipsis in the Earths motion is more a circle . . .”

The Baron says, “ C'est ici, hélas! que finit le fragment de cette lettre intéressante “ et remarquable. C'est bien dommage! car c'est précisément ici que le savant Lord “ entre en matière sur le mouvement des comètes, assurément il a devancé son siècle “ dans ses idées, car le noble Comte avait déjà laché, comme l'on dit, le vrai mot du “ guet, lorsqu'il a osé mettre en avant, que le chemin elliptiques des planètes montrait “ celui des comètes. On peut donc deviner avec quelque certitude, et par conséquent “ regretter les systèmes et les hypothèses que ce savant Seigneur aurait proposés, qui “ étudiait avec une si profonde application, et la plume à la main, les ouvrages épineux “ de Kepler.” It is clear from the expressions here used that the Baron had never seen more than he has given us of the letter, and therefore was not in possession of the earl of Northumberland's signature to it. He could only have collected from the contents that it was his, and it is not too much to say, that the internal evidence is decidedly against such a conclusion. It will be seen that the Baron supposes the letter to have been written in 1610, but the earl was prisoner in the Tower from 1606 to 1619[z], and during that time Wood[a] tells us, that Harriot was one of his "constant companions." Under these circumstances it was impossible for him to have complained of distance, or to say "again I want your conference, for I know an hour with you would advance "my studies more than a year here." The original of what the Baron has printed is not to be found among the papers which are known to exist in England, but as there is no reason to suspect that he has not faithfully given its contents, there was the greater satisfaction from being fortunate enough to find in the British Museum the conclusion which had escaped him. It is as follows:

" . . . . . . & in mars is more longe & in some of the other planets may be longer againe so in thos Commets that appeare fixed the ellipsis may be neere a right line.

" 3. His phansie of ecliptica media or his via regia of the sun. vnto wch the walke of al the other planets is obliq; more or lesse; euen the ecliptica uera vnder wch the earth walkes his yeares iournie; by wch he solues handsomelie the mutation of the starres latitudes & indeed I am much delighted with his booke but he is so tough in manie places as I cannot bite him. I pray write me some instructions in your next, how I may deale with him to ouermaster him for I am readie to take paines. te modo iura dantem indigeo, dictatorem exposco. But in his booke I am much out of loue with thes particulars. 1. First his maine and intollerable atechnies. whence derive thos maine & vncertaine assayes of calculation. 2. his finding fault with Vieta for mending the like things in Ptol. Cop. &c. but se the iustice Vieta speakes sleightlie of Copernicus a greater then Atlas. Kepler speakes as slightlie of Vieta, a greater then Appollonius whom Kepler everie where admires. for who soever can do the things that Kepler cannot doe, shalbe to him great Appollonius. but enough of Kepler let me once againe intreate your counsel how to read him with best profit, for I am wholie possessed with Astronomical speculations & desires.

" For your declaration of Vieta's appendicle it is so full and plaine as you have aboundantlie satisfyed my desire for wch I yeild you the thankes I ought. onlie in a word tell me whether by it he can solve Copernicus, 5 cap: of his, 5 booke.

" the last of Vieta's probleames you leave to speake of because (you say) I had a better of you, wch was more vniuersal and more easilie demonstrated, and findeth the point, E. as wel out of the plaine of the triangle giuen, as in the plaine. I pray here helpe my memorie or vnderstandinge, for although I haue bethought my selfe vsq; ad insaniā; I cannot remember or conceaue what proposition you meane. if I haue had such a one of you, tel me what one it is and by what tokens I may know it; If I haue not had, then let me now have it. for you know how much I loue your things and of all wayes of teaching for richnesse & fullnesse for stuffe and forme, yours vnto me are incomparablie most satisfactorie. if your leasure giue you leaue imparte also vnto me somewhat els of your riches in this argument.

" Let me intreate you to advise & direct this bearer Mr. Vaughen wher and how to prouide himselfe of a fit sphere; that by the contemplation of that our imaginations here may be releued in manie speculations that perplexe our vnderstandings with diagrammes in plano. he hath monie to prouide doe you but tell him wher the are to be had and what manner of sphere (I meane with what & how manie circles) wilbe most vsefull for vs to thes studies. after all this I must needs tell you my sorrowes. god that gaue him, hath taken frō me my onlie sun, by continual & strange fets of Epelepsie or Apolexie, when in apparence, as he was most pleasant & goodlie, he was most healthie. but amongst other things, I haue learnt of you to setle & submit my desires to the will of god; onlie my wife with more greife beares this affliction, yet now againe she begins to be comforted. let me here frō you and according to your leasure & frindshippe haue directions in the course of studie I am in. aboue al things take care of your health. keepe correspondence with Kepler. & wherinsoeuer you can haue vse of me, require it with all libertie. for I rest euer

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[z] The mistake with respect to Sion College has been already pointed out, (p. 22.) and when the letter was first printed in the Monat. Corr. (vol. VIII. p. 47.) the only designation used was "à Londres;" but still there is in this simpler address what must naturally suggest considerable doubt. The earl of Northumberland of those days (when he was himself in the metropolis) would never have barely directed his letter to "Mr. Tho^{s}. Harriot in "London."

[a] Vol. II. p. 300.

"Your assured & true frind to be
vsed in all things that you please
"Willm. Lowër."

"Tra'venti on Mount Martin
6. February. 1610."

"let me not make my selfe more able then ther is cause. I can not order the calculation by the construction you sent me of Vieta's, 3. probleme, to find the distances of C. &, D. &, B. frō the Apogeū or the proportion of *ia.* to, *ac.* the eccentricitie. I tooke Copernicus, 3. observations in the, 6. chap: of his, 5, booke. therfore helpe here once againe.

"To his especiall good
frind Mr. Tho: Harryot
at Sion neere London."

It is difficult to devise a reason for the Baron's having been led to imagine that the letter had ever been written by the earl of Northumberland. Some of this nobleman's papers [b] are still preserved at Petworth, and his handwriting is perfectly different from that of sir William Lower. There can however be now no doubt about the real author. Every thing is clear, and it even happens that the original seal remains attached to the paper, bearing the family arms [c], with a unicorn's head erased, for a crest, surmounting a knight's helmet. There is very little to be found in print about this remarkable man, and the whole of that little is not to be depended upon. This has been occasioned by his having been confounded with a nephew of the same name, a dramatic writer, of whom an account has been given by Wood [d]. What I have been able to collect about him at this distance of time cannot be much, but it will be found subjoined in another place [e]. His name [f] is new in the history of English astronomy, but it deserves to be recorded.

Several of the topics in this most curious letter are evidently continued in that of June 1610 [g], and give further precision to some facts connected with the state in which science was at the time when it was written. It shews that telescopes were made in London as early as Feb. 1610, and corroborates what has been said[h] on the earlier observations of the moon: but there are points of still more importance, for which much information may be derived from it.

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[b] None of them are on scientific subjects.

[c] Sable, a chevron between three roses argent.

[d] Ath. Ox. vol. III. p. 544.

[e] See note I.

[f] He is not noticed in Sherburne's Manilius.

[g] P. 25.

The letter was republished in vindication of the high importance which the Baron de Zach attached to Harriot's papers, and in confirmation of some remarks on Dr. Robertson's publication of his Reports upon them. The present statement has certainly exceeded the bounds which I first anticipated, and possibly is already too long; but this portion of the Baron's reply must not be left unnoticed. It contains many particulars, of which the lapse of years must have weakened his recollection; but as they are stated without hesitation, the present discussion could not (as I hope it will) be final, if some of the mistakes in them were not distinctly pointed out.

Before however we enter upon the particular objections which the Baron brings forward, it may be right to notice an impression which must be received by every reader who is unacquainted with the particulars of the case. After mentioning (p. 105.) lord Egremont's supposed gift of Harriot's papers to the University, as if it had taken place in 1784, or soon after, he goes on to say, (p. 106.) " Il s'agissait dans le tems d'en publier des " extraits, mais le docteur Robertson, professeur d'astronomie en cette université, avait " declaré en 1802, que ces manuscrits ne contenaient rien qui fut digne de l'attention " publique." It would not be worth while to notice that Dr. Robertson became professor of astronomy only in 1810, if it were not that the circumstance might lead to a suspicion of his opinion, as here referred to, being of a still later date than 1802. Now the idea at present conveyed by the Baron's statement is, that the papers remained at least eighteen years at Oxford before any determination was made about what was, or was not, to be done with them. After all that has been said, it will be unnecessary to repel an insinuation, which it is impossible that the Baron could have intended to convey. He must have known that for the greater and most important part of this period, (for the first ten years,) the papers were wholly and solely at his own disposal; and, although I would not speak lightly of the time and care which would be required to ascertain how far they were fit for the press, still if this preliminary step was long before it was executed, he must have known to whom the delay was principally to be attributed. The expression of " des extraits" must likewise convey an inaccurate idea. With a view to this, the letter is printed in which the Baron made his formal proposals to the Delegates of the Press in 1786[i], and it evidently appears from it that the design was any thing rather than the mere selection of " some extracts" of particulars, which might be interesting in themselves, or calculated to convey an idea of what the papers contained. I say this to prevent mistakes, and I have no wish to impute blame to the Baron for what he may simply have left undone in the business. He embarked in it of his own freewill, and if called off to other objects would naturally be desirous of giving up the undertaking. He had imprudently raised expectations which could not be fulfilled; but those, who had most ground for complaint against his withdrawing, were the Delegates of the Oxford Press, who had been induced, at his request, to take a share in the publication. They, however, would have been the last to insist on a contract which had become inconvenient, or even unpleasant to him; and, in the exercise of reciprocal good feeling, they ought to have been the last, on whom he should have endeavoured to fix any blame for not pursuing what he had himself abandoned. Whether he has been sufficiently cautious in this respect, and whether he has had good ground for what he brought forward, is now to be examined. He reduces his remarks, on what he thought had been improperly neglected, into a series of questions; and that the reader may be best able to judge of them, I shall give each in his own words, with such observations as they suggest.

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[h] P. 20.

[i] See note B.

"[k] Il est très vrai, comme le remarque fort bien le docteur Robertson, que tous les manuscrits de Harriot ne méritent pas l'impression, et n'avaient pas été écrits dans cette vue; cependant on aurait bien pu en donner des extraits, surtout de ses observations astronomiques. Par exemple, Harriot observait avec une grande application les satellites de Jupiter tout-nouvellement découverts . . . . . Ne serait-il pas curieux, & même glorieux à la mémoire de Thomas Harriot, de faire voir, ainsi que nous l'avons fait il y a 38 ans, qu'il avait été un des premiers astronomes de l'Europe, hors l'Italie, qui eut, non seulement observé, mais aussi calculé, les révolutions périodiques de ces lunes autour de leur planète?"

If an answer was merely intended to the individual writer, it might be sufficient to suggest, that no one could have a right to blame others for not doing what he says that he had himself done nearly forty years before, and which, if properly executed, would not require to be repeated. But without dwelling on the introductory matter, it must be remarked that the two parts of the last sentence are not strictly in accordance with each other. In the early accounts (see note A) to which the Baron refers, he distinctly claimed for Harriot the honour of having discovered Jupiter's satellites even before Galileo, but in this instance he only speaks of him as among the first "hors l'Italie" who paid attention to the subject. I am not aware of any place in which the Baron has expressly mentioned this alteration in his views of the question; I may however have overlooked it, nor have I any wish to deprive him of the argument (as far as it goes) in favour of his not being guilty of intentional mistatements. Indeed he could never have urged others to the publication, or even the examination of these papers, if he had been aware of all the misconceptions which he had formed respecting their contents; but this very correction is an additional proof of his having originally given reins to his imagination, and having trusted himself to two of the most fallacious guides upon which human beings can depend——impressions at first very hastily collected, and the powers of recollection without reference to original documents. The same circumstances will be found in many of the following questions, and it may therefore be a subject of regret that the real state of the case was not more fully developed at an earlier period. It must however be recollected, that although much of this later inaccuracy might have been discovered without an examination of the original papers, yet without them no one could have ventured on a general answer to the Baron's allegations, and a special application for the use of them was what few could think themselves entitled to make. When I went to Petworth, it was only in hopes of seeing what was there; I never anticipated the indulgence of being allowed to bring them away with me, and to print any parts which I might think desirable to be laid before the public.

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[k] p. 108.

> "Ne serait-il pas curieux, & même glorieux à la mémoire du comte de Northumberland, et à celle de Thomas Harriot, en faisant voir qu'en 1610, ils avaient déjà à Londres des lunettes d'approches, qu'ils appellaient en anglais 'perspective cylinder,' tandis qu'en 1622 on doutait encore en France de l'existence de cet instrument 'que l'on ne croyait pas légèrement dès deça' comme s'exprimait le célèbre Peiresc?"

Whatever was honourable in such respect for the earl of Northumberland, was still more honourable for an individual like sir William Lower; there can be no occasion therefore to cavil about this mistake: but it is remarkable that the Baron, when he had not seen the date of the letter, should have fixed so exactly upon 1610 for the time when it was written. It has been seen [l] that the case is still stronger than he makes it, for Harriot had a telescope in July 1609, but the information, by which the honour of the earl of Northumberland was to be vindicated, depended (it must be remembered) entirely upon a letter, which the Baron had kept in his own hands, and so far had he been from sending it with the other papers to Oxford, that its contents were not known till he published them in 1803, nor has any thing even now been discovered about its very existence. That telescopes were unknown at Paris in 1622 is in contradiction to Gassendi's [m] account of Peiresc's having been supplied with them from that very place at a much earlier period. Indeed, in another place[n], the Baron suggests the possibility of some mistake in this date; but that is, after all, not the true solution of the difficulty, for Peiresc [o] expresses no doubt about the existence of telescopes in general, but of some long ones, by which it was said that writing could be read at the distance of a league.

> "Ne serait-il pas curieux, & même glorieux pour la nation à laquelle nous devons la connaissance des véritables lois de la nature, et de toute la mécanique de l'univers, de voir que Harriot avoit pensé & enseigné long tems avant Kepler que les orbites des planètes étaient elliptiques?"

There is nothing to which this can refer but to sir William Lower's communication, which the Baron had kept to himself, and which after all will not by any means support the implied accusation. "Utinam tam facile vera invenire possim quam falsa convin-"cere [p];" it is clear from the indisputable testimony of this valuable letter that Harriot had shewn, from his own investigations, "that the motions of the planets were not perfect "circles;" but we have no evidence of his having advanced further. This indeed was a considerable step, but it must not be put in competition, much less be considered as identical, with the actual discovery of Kepler's great law of the universe.

> "Ne serait-il pas curieux, & même glorieux pour cette même nation, en lui faisant voir qu'en 1610 un compatriote de Newton avait déjà proposé de calculer la marche inconnue des comètes dans des orbites elliptiques, tandis qu'en 1619 Kepler lui-même dans son traité "de cometis libri "tres" la croyait encore rectiligne?"

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[l] P. 20.

[m] P. 28.

[n] p. 124.

[o] Camdeni Epistolæ, p. 333.

[p] Cic. de Natura Deorum, lib. I. c. 32.

Sir William Lower's suggestion [q] with respect to the orbits of comets might almost be ranked with some of Newton's queries. To have extended to them by analogy the elliptical theory of the planets, was in the true spirit of philosophy; but if the Baron's question is meant to imply that there was any plan of reducing it practically to calculation, it may be right to explain that no traces are to be found of such an intention. The time indeed had not yet arrived for the complete investigation of this important conjecture; but there is a letter of Dr. Wallis to Oldenburgh, dated Jan. 21, 1665.[r] in which he speaks of the opinion which Hevelius had maintained of the rectilinear paths of comets; and he adds, "Since conjectures about such hypotheses are now stirring, I thought it not amiss "to revive that of Mr. Horrox, which I find among his papers. In some of his letters "to Mr. Crabtree, he intimates his conjecture of their issuing forth of the body of the "sun by a strait line. Afterwards upon examining the motion of some comets, he "addeth to that of their coming from the sun's body some curvity of motion arising "from it. At length he seems to have pitched on such an hypothesis, as the figure in-"closed represents, making it issue out of the sun's body, and by an elliptical figure (or "near it) to return thither again; according to which he hath traced the motion of that "of 1577, (according to Tycho's observations, as I suppose.) But this paper being his "own hand, and (for ought I see) all that is remaining concerning this hypothesis, I "desire that it may be carefully preserved." Wallis, it may be recollected, was engaged in arranging and collecting the papers for Horrox's Posthumous Works, which he published in 1672. In that work there is [s] a letter to Crabtree, in which mention is made of comets; "quod nempe ex sole prodeant omnes, sed et a sole circumducuntur: æqua-"liter autem, an (quod verisimile videtur) inæqualiter, pro varia a sole distantia, non "ausim definire." The diagram, to which Wallis refers, is preserved with his letter in the archives of the Royal Society. The orbit which it delineates has an obtuse cusp at the sun, and is such as it would be impossible for the comet to have really described; but it proves that Horrox had thought of comets possibly revolving in curves which returned into themselves—an idea which, if the life of that extraordinary man had been prolonged, might have ripened into a considerable advance towards the truth, although nothing less than Newton's power was adequate to its full developement.

The Baron indeed anticipated systems and hypotheses [t] which were lost to us by his only being able to present the public with the beginning of sir William's letter. The termination of it however does not contain what was looked to with so much confidence. "Those comets which appear fixed" may possibly allude to phænomena (if any such should be observed) like the famous star of 1572 which was seen in Cassiopeia; but this is the only application of the theory which is proposed; and it may be remarked, that the proposal seems principally to have been brought forward because it does not practically introduce any wide departure from the rectilinear hypothesis.

In his letter [u] to the Delegates of the Oxford Press, the Baron says distinctly that Harriot had assigned a parabolic path to the comet of 1607—"apparet ex celeb. viri M.sptis "illum jam cometæ anni 1607 viam parabolicam assignasse." This, if the account were accurate, would have been a splendid instance of astronomical advance in the beginning of the seventeenth century. But how does this great fact appear from Harriot's manuscripts? not from any particulars [v] to that purpose having been found among them, for the Baron himself tells us in another part of the same letter, that "in Harrioti chartis "nil aliud quam hujus cometæ distantiæ a fixis variis ad nullum revocatæ calculum "extant." Neither can an argument be drawn from any subsequent use of the theory. Harriot's calculations for the comet of 1618 are all entire in the papers which are preserved at Petworth, and his method is very simple. He calculates the distances not in a parabola, but in a great circle, for the intervals between the several places in which the comet had been observed. He gives a tabulated view of the results, and compares the sum of all these quantities with the distance, still in a great circle, between the first and last places in which the comet was observed. This very table has been printed by the Baron himself [u], though he has not arranged the result in the most perspicuous manner. It should be as follows:

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[q] P. 43.

[r] Birch's Hist. of the Royal Society, vol. II. p. 11. [s] p. 321. [t] P. 43. [u] See note B.

"Summa (of the several distances)	66°	52′	22″
"Distantia a primo loco ad ultimum in circulo magno	66	35	50
Differentia	0	16	32"

Harriot likewise estimated the inclination of the orbit by calculating the angle which the ecliptic would make with a great circle passing through the first and last places in which the comet had been seen. All this was an effort of great industry, but exhibits nothing beyond the knowledge of the time in which he lived, and applied, after all, to the apparent, not to the real, path of comet.

"Ne serait-il pas curieux, et même glorieux, de faire observer, et de demonstrer par les papiers de Harriot, que ce grand et véritable génie avait long-tems avant Ghetaldi fait des expériences sur la pesanteur spécifique des corps?"

This fact is certainly very curious and interesting, but there are no traces of it in any of the papers which the Baron sent to Oxford. There are however a sufficient number bearing on the point in the British Museum, and one which corroborates in a very remarkable manner the praise which sir William Lower so justly bestows upon his friend. Ghetaldi published his Promotus Archimedes in 1603, and one of Harriot's papers connected with specific gravities, is dated Sept. 22, 1601. This was not certainly "long-tems avant," but it agrees perfectly with Ghetaldi's "within a while coming out "with it in print."

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[v] There is one half sheet of paper containing a curve, which resembles very much a semiparabola. On the axis of it are taken equal parts, and against the points of division are affixed dates from the 15th of Nov. to the 23d of Dec., which last is at the vertex of the curve. This may have suggested the via parabolica to the Baron, but it cannot represent the orbit of a comet, because it makes the abscissæ proportionate to the times; and if it had been intended for such a purpose it could not have been for that of 1607. which Harriot did not observe after the 22nd of October. Against the 15th of November is written "London & Syon," and this is about the time when the comet of 1618 was seen there, but it had then passed its perihelion. Kepler has a curve which is intersected by the rectilinear path of his comets, and we have a straight line in the same manner cutting Harriot's in two points, which answer to the places from whence perpendicular ordinates would be drawn to the 21st or 22nd of Nov. and the the 13th or 14th of Dec. There is no further explanation, but it is not improbable that this, after all, is only a graphic trial to represent a portion of the figure according to the same method with Kepler; and if so, Harriot must in 1618 have still endeavoured to calculate for the comet in a rectilinear path. It is right to add that the curve much resembles that which Kepler gives for 1607, but is very different from that which he draws for 1618, and which has more resemblance to an hyperbola than a parabola.

[u] Bode, Jahrbuch, Supp. I. p. 34.

"Ne serait-il pas curieux, et même glorieux à la mémoire de ce grand mathématicien,...... de faire voir, que près d'un siècle et demi avant James Dodson, il avait déjà calculé des tables antilogarithmiques pour 100.000 nombres?"

The account of this antilogarithmic canon is in Wood's Athenæ Oxon.[x], and he does not give the least hint of Harriot's having had any share in forming it. He distinctly says that it was calculated by Warner for all numbers to 10000, and completed many years after by Dr. Pell for the remaining 90000. Wallis, in his first English Treatise of Algebra, (1685.) does not mention this work in his XIIth chapter, but in that part of the Latin translation, (which forms the second volume of his Works, 1693.) he introduces Wood's account, and adds one or two circumstances from his own knowledge. He cannot be suspected of want of interest in the subject, but he speaks with great doubt of Harriot's having even had any share in originating the undertaking—"Canonem "illum nescio an inchoaverit D. Thomas Harriot."—He adds, "opus illud memini "me vidisse (et quidem solummodo vidisse) inter alia, sive Harrioti, sive Warneri sche- "diasmata."

"Ne serait-il pas curieux, et même glorieux à la mémoire de Harriot, de faire voir qu'il avait observé la fameuse comète de 1607, dite la comète de Halley, et dont les observations nous ont donné la véritable orbite elliptique de cet astre remarquable?"

In answer to this inquiry, it is only necessary to refer to what has been already said on the subject in P. 41. There was no reason for the author's reflecting upon others for not doing what he had professed to have done before them.

"Enfin, ne serait-il pas curieux, et même glorieux pour un compatriote de Harriot, d'entreprendre une nouvelle révision de ce fameux procès entre lui et Descartes, sur la priorité de plusieurs découvertes algébriques; contestation à-peu-près semblable à celle entre Leibnitz et Newton au sujet du calcul différentiel et intégral?"......

To help in establishing the fair fame of Harriot would be a source of the highest gratification to me. He was not only a countryman, but a distinguished member of that university to which I have myself the honour of belonging. If truth hath obliged me to shew that his astronomical observations are not of that high character which they have been supposed to possess, he can well spare what does not belong to him. There is no degradation in being second to Galileo, and there is much praise to which he is most justly entitled. He certainly made a very early use of the great invention of telescopes; he carefully examined some of the most curious phenomena, which they enabled him to observe in the heavens; he took great pains to ascertain the laws of the planetary motions, and was not deterred from the necessary calculations, even when, before the invention of logarithms, he was obliged to work by methods of a more operose description. Could I have continued the inquiry to an examination of his other pursuits, I probably should have met with results more consonant with my wishes. Sir William Lower's letter encouraged this hope very strongly: he appears to have shared in Harriot's studies, and to have been intimately acquainted with them: his testimony therefore is of great weight to the priority over Vieta[y], and I sincerely hope that some one who has leisure, and to whom a residence in London may give opportunities which I do not enjoy, will pursue this part of the inquiry, by thoroughly examining the papers in the British Museum.

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[x] Vol. II. p. 302.

Of those papers which were sent to Oxford the description of the geometrical part may be seen in Dr. Robertson's Reports; and this was a subject on which he was most especially fitted for forming a good judgment; not only from his general knowledge, but from his having recently published his first work on Conic Sections, which contains a valuable history of that branch of science. Of the algebra he hardly says any thing: he mentions indeed the name of Vieta, but gives no particulars, probably from there being so little to remark upon. The whole which the Baron selected on this subject was confined to sixteen half sheets of foolscap. Thirteen of these are entitled, "de numerosa "potestatum resolutione[z]," and contain problems similar to those in the "Exe"getice Numerosa" of the "Artis Analyticæ praxis." Among these, several of the examples are marked as Vieta's, and this shews how necessary it is to be cautious in reasoning on what belongs to different individuals. The occurrence of this name, if we had not sir William Lower's testimony to correct it, would certainly give the impression of Harriot's having probably borrowed more than the examples with which he illustrates his methods. The remaining three papers contain the resolution of some biquadratic equations, in the way devised by Ferrari. It seemed worth while to have one of these lithographed in Pl. V. Sup. No one who may be inclined to examine the whole of it can be impeded by any serious difficulty, and the object for which it is now published may be evident to the most casual observer. M. Montucla[a] is very particular in arguing "qu' Harriot n'eût qu'une ideé peu developpée des racines négatives." This is a point which he considers that he has completely proved, and no objection can be properly brought against his argument from what was then unpublished. But it is impossible for any one to look at the example here given without seeing at once that the opinion is evidently erroneous. A modern algebraist would be less diffuse, and write his roots with more compactness, but he must bring them out upon the same principles, and could add nothing in this respect to the results.

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[y] P 43.

[z] One of these is curious, from containing the same numbers as the "Anticipationis exemplum," in p. 143 of the "Artis Anal. Praxis."

[a] Histoire des Math. vol. II. p. 107.

NOTES.

NOTE A, p. 17.

IN the Memoirs of the Royal and Imperial Academy of Bruxelles, (vol. V. 1788,) the Baron de Zach published a paper on the Georgium Sidus: he introduced into it the mention of discoveries that he attributed to Harriot, and added a note, (in p. 24,) which contains the following passage:

"C'est dans l'été de 1784 que je fis cette découverte curieux. Ayant passé cette saison sur les terres de Milord Egremont à Petworth, dans le comté de Sussex, je trouvai dans la bibliothèque de son chateau de vieux manuscrits du célèbre Thomas Harriot. M. le comte de Bruhl envoyè extraordinaire de la cour de Saxe à la cour de Londres, à qui ce trésor étoit connu, m'en avoit donné la connaissance. Parmi ces papiers reellement précieux, je trouvai que Harriot avoit observé les taches du Soleil et les quatre satellites de Jupiter avant Galilée. La découverte de ces manuscrits n'est pas seulement intéressante et curieux pour l'histoire de l'astronomie, mais infiniment utile et précieux pour les différentes observations qu'ils contiennent, et que je compte de donner un jour au public."

Count Lucchesini inserted some remarks on this statement in Tiraboschi, Storia della litteratura Italiana, in which he says,

"Aspetto con ansietà che il Sig. Zach dia in luce questi manoscritti; ma credo che intanto
" ci sarà lecito di dubitare un poco, che forse quelle osservationi dell' Harriot non sieno state
" esaminate abbastanza, nè ben consideraté i tempi, nè quali furano fatte [a]."

NOTE B, p. 18.

[b] " Illustrissimis, clarissimis, et doctissimis viris celeberrimæ ac antiquissimæ academiæ Oxoniensis, preli curatoribus, Franciscus de Zach, ejusdem ecademiæ legum doctor [c], serenissimi ducis Gothani astronomus aulicus, scientiarum academiarum quæ Bruxellis, Lugduni-Galliæ, Massiliæ, et Divioni florent, socius—Salutem et felicitatem."

"Reperienti mihi (annus est) et perlustranti autographa summi et sagacissimi mathematici et astronomi nunc iterum in literarum Republica redivivi Thom. Harrioti injectum est illico desiderium moliendi opus publico jure dignum, in quo ad multorum honorum vindicationem scripta tanti viri una cum controversiis et commentationibus ad ea pertinentibus quanta possem eruditione colligerem et ex oblivione ad apricum producerem.

"Accinxi me ad opus hoc explorando summa cura manuscripta quotquot inveneram, perlectisque quotquot nancisci potui de authoris vitæ et manuscriptorum argumentis voluminibus; sed quo tempore et quo auxilio in lucem proferretur, nulla erat, post tot tantosve conatus spes relicta, nullum relictum consilium.

"Postquam vero, vos, viros illustrissimos doctissimos almæ et celeberrimæ academiæ rei typographicæ curatores, non tam patronos ac vindices, quam æstimatores et judices nactus sim, qui ad

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[a] Vol. VIII. p. 358.

[b] This document is very long, and contains much which does not bear on the present purpose. It would have been desirable not to have printed the whole of it; but the reader might not have been satisfied of the fairness with which any part had been omitted.

[c] In Feb. 1786 the complimentary degree of honorary Doctor in Civil Law had been conferred on the Baron.

vulgandum opus sumptus summa cum liberalitate et munificentia suppeditare decreveritis, id mihi officii credidi dari ut quæ publicatu digna, luce non indigna mihi viderentur, paucis his vestro submitterem judicio, sic non modo monumentum in summi ingenii virum e gremio vestro natum æternumque meæ in vos observantiæ pignus habebitis, sed me ipsum et nuperrime gremio vestro adscriptum de ejus honore et gloria summopere sollicitum perpetuo retinebitis.

“ Valete, viri illustrissimi, clarissimi, doctissimi, cum ad amplitudinem celeberrimæ academiæ, tum ad scientiarum et bonarum artium incrementum et præsidium diu felices. Dabam Londini Idibus Aprilis MDCCLXXXVI.

“ I. *Authoris Vita.*

“ Autographa Thom. Harrioti edituro primo omnium in limine operis vita viri omnis eruditionis et communis literarum sui ævi cultoris contexenda venit. Sic non solum iis qui ortus et progressus artium et inventionum curiosius investigant, verum etiam iis, qui historiam et incrementa scientiarum, imprimis algebraicæ scientissime perscrutantur, ex omni parte erit consultum, quo et fiet, ut æqua disquisitione merita inventori tribuatur cuiquæ gloria. Sat erit mihi hic indicasse actum non agi si vitam hanc ad modum Gassendi in vitam Purbachii, Regiomontani, Copernici et Tychonis conscribendam susceperim cum ex fontibus per sesquiseculum reconditis et ab omni hominum cognitione remotis, doctissima felicissimaque inventa solertis ingenii viri in lucem nunc primum proferre valeam.

“ II. *Observationes Astronomicæ Cometæ* 1607.

“ Docet philosophia Newtoniana cometas equidem ac planetas attractionis vi, quæ in ratione duplicata distantiarum reciproca a sole est, in orbibus ellipticis circa solem in communi foco positum revolvi; Discrimen unicum quod cometarum inter et planetarum orbes interest illud est, ut illi sint ellipses protractæ seu ingentis excentricitatis, isti ellipses ad circulos quam proxime accedentes, jam vero in definiendis planetarum semitis orbes tanquam ellipses, quales revera sunt, spectantur, illorumque formæ ex observationibus definiuntur, atque porro axes et cætera elementa calculo haud ingrato deducuntur; Non ita res est in cometarum determinandis orbibus namque inextricabilis immensique esset laboris illorum orbitas per ellipsin ad unguem investigare, commodiorem sibi itaque aperuerunt astronomi recentiores viam in cometarum motibus supputandis, ausus est jam summus astronomus Braheus et post hunc Mæstlinus cometæ anni 1577 circularem assignare orbitam; ausus est celebr. Keplerus cometæ motum anni 1618 rectilineum facere, ast quis astronomorum primus fuit, qui cometarum motus parabolæ adaptavit Hevelium inter et Doerfeldium lis penes judicem hæret, Litem vero hanc sagacissimus mathematicus et astronomus Thom. Harriotus componit, apparet enim ex celeb. viri M.sptis illum jam cometæ anni 1607 viam parabolicam assignasse. Hevelius de cometarum parabolico motu tantummodo in cometographia anno 1668 typis edita verba facit, Doerfeldus primum in cometam anni 1681 parabolæ suppositionem confinxit, ab hoc tempore omnes astronomi tum circulares tum rectilineos imprimis Newtoniana doctrina traducta ellipticos neglexere motus, qui ob prope infinitos calculos et prorsus ingratos vix Herculeo labore tractabilis esset, feliciter vero et mirabili cum circumstantiarum consensu evenit, ut cometarum orbes in solis vicinia, hoc est in systemate nostro planetarum decursu vix a parabolicis abludentes, cum parabolæ unicæ curvæ tetragonismo mirifice convenirent, quod et methodum expeditum et simplicem suppeditat illorum orbitarum elementa non operose adeo calculo ex datis geocentricis observationibus depromere.

“ Quandoquidem cometa quam diu nobis conspicuus per parabolam apprime possit representari, attamen ut per se liquet, periodicum tempus totius revolutionis per orbem nequaquam per parabolam obtineri potest, per ellipsin vero, ultra immensum calculorum farraginem parum admodum accurationis inde spectare perito astronomo fas est, siquidem vel minimus in observando irrepserit error totius ellipseos formam ultra modum immutat, protendit vel contrahit, ut nil exinde certi colligi possit; ad stabiliendum itaque cometarum reditum et tempus revolutionis periodicum ad aliam confugiendum erat methodum, nam theoria Newtoniana perpetuum cometarum in orbem recursum postulabat, obtulit se illico collatio cometarum orbium et illorum elementorum inter se si enim invicem vel consentiant vel parum discrepent, concludendum jure erit, unum eundemque esse cometam orbesque eorum specie tenus pro iisdem haberi posse, et si præterea tempora post quorum lapsum cometa ad perihelium iterum regreditur, inveniantur æqualia, nulla tunc sane supererit incertitudo aut ullum de hujus cometæ identitate dubium, indeque motus revolutionis periodicus citra omnem hæsitationem colligi poterit.

"Talem cometarum orbium mirabilem consensum primus detexit, primus exinde reditum cometæ prædixit homo Oxoniensis in alma hac academia geometriæ professor Savilianus Dr. Edmundus Halleius Societatis Regiæ scientiarum quæ Londini floret e secretis, immortali quoque Flamstedio in specula astronomica regia Grenovicensi successor meritissimus. A° 1705 clariss. vir ille insigne hoc inventum publici juris fecit, simulque Synopsin elementorum 24 cometarum in orbibus parabolicis tradidit, et per orbium congruentiam potissimum ostendit cometam in una et eadem revolvi ellipsi qui A° 1305, A° 1380, A° 1456, A° 1531, A° 1607, A° 1682 ad solem maxime appropinquavit, quapropter hujus cometæ reditum audenter definire cœpit, assumpta itaque periodo media $75\frac{1}{2}$ annorum, quæsivit orbitam ellipticam, habitaque ratione deflexus a vi perturbante Jovis procedentis in cujus vicinia cometa per plures versabatur menses, tandem per calculum maxime laboriosum conjecit, posteritatemque monuit, ut reditum ejus circa initium anni 1759 invigilaret, quemadmodum reipsa contigit, et de quo summo ingenii humani et astronomiæ conatu non injuria gloriatur Britannia.

"De hujus cometæ motu, periodo, perturbatione, incredibili industria et ingenii sagacitate egit summus geometra Clairautius, qui in eximio edito opere 'Theorie du Mouvement des cometès: immani calculorum labore omnes illius cometæ perturbationes, et inde ortam temporis protractionem ad stupendam revocavit veritatem, ex sola Saturni attractione retardationem trium mensium invenit, sed magnæ adhuc restat inter astronomos de mole Saturni incertitudo facit illam Newtonus $\frac{1}{3021}$ posita hac solis $= 1$. Invenit vero clar. de la Lande ex quarto ejus comite $\frac{1}{3393}$ mediam vero ex quinque $\frac{1}{3927}$. Putat itaque eximius Geometra D. de la Grange ex collatis Harrioti in cometam anni 1607 quadam accuratione habitis observationibus cum iis quæ annis 1682 et 1759 institutæ fuerant, tempus periodicum et ex ejus inæqualitate et protractione quæ ad annum integrum et menses 7 assurgit, Saturni molem (habita æqua ratione de Veneris ac Jovis cognita actione) posteriori via colligi ac comprobari posse.

"Ex prædictis itaque apparet qualis quantæque notæ cometa anni 1607 sit, et quamquam Hallei sententia imprimis in cometis annorum 1531 et 1607 fulciatur, tamen observationes Apiani et Kepleri in hos cometas oculorum fide et crasse institutæ ad tam subtile negotium enucliandum haud satis idoneæ sunt. Nam Petrus Apianus cometam anni 1531 a die 13° Augusti ad 23tiam tantummodo ejusdem mensis observavit. Keplerus et Longomontanus vero cometam anni 1607 a Sept. 16 ad Octob. 19 observarant et quidem nulla subtilitate ut ipsius Kepleri verba (de cometis, pag. 30.) 'nec enim opus est subtilitate cum observatio sit crassa' ita quidem ut elementa inde deducta multum a vero abludere possint. Affirmat Longomontanus in Appendice ad Astronomiam Danicam cometam hunc non eluxisse ante diem 25tam Septembris siquidem die 24ta observavit Jovem et nihil novi in cœlo vidit quoniam vero media oppositio solis cum Jove die 25ta valde consentaneum putat, ut cometa tunc primum fulserit quo tempore fuit conjunctio Mercurii cum Sole et trinus adspectus Saturni cum Marte quæ ratio tunc temporis superstitionem subolet.

"Refert Keplerus in libro suo de cometis, pag. 4. cometam de 23^{tia} Septb. animadversum fuisse, sibi vero (pag. 25.) primum visum ait Pragæ die 26ta Septb. hora media post octavam. Ex Thom. Harrioti M.sptis pretiosissimis vero apparet cometam hunc primum in Anglia die jam 13^{a}. Sept. a quodam Torporleio Æn.[d] Nas. Coll. Oxon. socio, amico familiari Harrioti viro in omni mathematum doctrina versatissimo conspectum fuisse, primam vero astronomicam et exactam observationem Oxonii instituit quidam Standish cujus nomen forsan illam in qua inest obscuritatem meretur, a die 15 Sept. ut epistola Torporleii ad Harriotum die 30 Sept. data habet, instituit Torporleius observationes decem, Harrioti accuratæ observationes sunt a 21 Sept. ad 22 Oct. usque, ubi disparuit cometa. Modus illi observationis ille distantiarum a fixis fuit, et quo Tycho, Hevelius et Flamstedius relicto veterum astrolabio ad astrorum longitudines et latitudines determinandas, usi sunt & qui primæ astronomiæ recentioris fundamentum fuit. Maximum itaque operæ pretium erit Harrioti observationes ad recentiores calculos nostros revocare quod maximo ut cum fructu fiat, in æquam necesse erit observationum disquisitionem et correctionem inire, aggrediar itaque opus: Reductione distantiarum apparentium ad distantias veras, reformatione locorum fixarum a quibus cometæ distantiæ captæ erant ad epocham observationum, ex catalogis fixarum recentissimis, adhibita æquinoctiorum præcessione, luminis aberratione, axeos telluris nutatione, cometæ in distantiam parallaxeos annuæ et refractionis astronomicæ effecta, correctis insuper erroribus in ipsis observationibus ex collimationis difficultate et ex transversalium inæquali subdivisione in instrumenti limbo commissis. In Harrioti chartis nil aliud quam hujus cometæ distantiæ a fixis variis ad nullum revocatæ calculum exstant, sed dubium mihi nullum est virum clarissimum calculos hos instituisse, quoniam illos in cometam anni 1618 in optimo ordine digestos reperi, verisimilius est dispersas aut perditas nonnullas esse chartas, ast etiamsi exstarent prædictis indigerent correctionibus, nam Harrioti tempore non alius nisi Tychonis catalogus fixarum exstabat, quo igitur usus est ad longitudines et latitudines cometæ ex observatis distantiis depromendas, et quamquam Tycho asserit positiones fixarum ad veritatem minuti primi circuli maximi accedere tamen accuratissimus Flamstedius ostendit (Historia cœlestis) Tychonem sæpe cum in locis tum latitudinibus duos, tresve, interdum quatuor aut quinque totos scrupulos a vero aberrasse. Refractiones quoque tunc temporis primum a Tychone repertæ multum a vero ab-erant, supponit enim Braheus altitudines astrorum ultra gradus 20 non amplius infici astronomica refractione quandoquidem revera $5'\frac{1}{2}$ adhuc sit. Solis parallaxis ultra modum quoque assumpta erat, nam tribus constare minutis supposuit. Reformatio horum errorum post Harriotum evenit, nam A° 1660 Clar. Dominicus Cassini primus ostendit refractionem augeri, imminui debuisse solis parallaxin horizontalem, accedit ad hoc Tychonem obliquitatem eclipticæ plus justo majorem 2′ 30″ assumpsisse.

"Quæ tandem organa et instrumenta attinet quibus clar. Harriotus usus est in observationibus suis peculiarem materies hæc meretur disquisitionem, ex manuscriptis apparet illum tum armilla sicut in Brahei Astronomiæ mechanica instauratæ descriptam vides, tum Baculi Astronomici quem Angli 'staff' vel 'cross-staff' nuncupant, et quem apud Bernardum Waltherum, pag. 11. descriptum habes tum Radii Astronomici de quo Thom. Diggesius in Ala seu Scala Mathematica, pag. 2. post partem practicam, passim usum fuisse. Observationes vero his instrumentis institutæ incertæ evadunt, tum ob humani oculi excentricitatem in collimando, tum ob transversalium inæquali divisione, quibus accidit ut præcisio non raro citra 10′ aut circa sit; teste Hevelio in Machina sua Cœlesti docemur nobilem Danum in primæ magnitudinis stellarum distantia trium minutorum errores commisisse quos clar. Flamstedius soli collimationis errori adscribit. Notandum quoque est quod anno 1653 transversalium æquatio in divisione instrumentorum primum incœpit cogitari; Norwoodius in libro suo "Military Architecture" pag. 81. et post illum quidam Wm. Leybourn in opere cui titulus "Compleat Surveyor," lib. II. pag. 39. hanc monent divisionum inæqualitatem, tradit primus trigonometricam alter geometricam seu mechanicam demonstrationem, ad quod tamen si attentionem convertes, non ad amussim, sed multum poteris instrumenti errores imminuere et observationes pro re nata castigare ad majoremque producere accurationem, quibus denique in æquiorem problematis indagationem inire licebit. Eodem ferme modo purgandæ veniunt.

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[d] This is a mistake, Torporley was never fellow of Brazen-nose College, see P. 61; and the observation was made by sir William Lower. See Bradley's Misc. Works, p. 513. note g.

III. *Observationes Cometæ anni* 1618.

"Qui hujus anni est tertius, clarissimus et caudatissimus, apparitionis dies est Jovis 29ª. Novb. hora media post 6tam ex Harrioti vero schedis apparet se illi adspiciendum obtulisse jam die 15 Novbris quod his habet verbis, "It was seen at Syon the 15th Nov. I did not observe it before the 20th." Refert Struyckius (pag. 262.) cometam hunc in India prope Jacatra die 7ª Nov. conspectum fuisse, quod in dubium vocat clar. Pingreius in Cometographia, tom. II. pag. 8. et secundum loco tertii acceptum fuisse suspicatur, tamen in Harrioti adnotationibus reperi cometam hanc ante hebdomadas tres in Indico mari navigantibus apparuisse. Kepleri et aliorum observationes sunt crassæ et dissidentes, Harrioti observationes vero sunt accuratiores et majorem orbitæ tractum exhibent, ac propterea orbitam a Halleio definitam ad majorem perducent perfectionem atque cum cœlo consensum.

"Volumen primum edituro tria hæc argumenta in votis sunt—
sed de hoc judicent viri clarissimi doctissimique."

NOTE C. p. 17, 29, 56.

"Translation of a paper given by Dr. Zach, astronomer to his highness the reigning Duke of Saxe Gotha, member of the Imperial Academy of Sciences at Brussels, of the Royal Academies of Sciences in Lions, Dyon, and Marseilles; printed in the Astronomical Ephemeris of the Royal Academy of Sciences at Berlin, published for the year 1788."

"I here present to the world a short account of some valuable and curious manuscripts, which [c] I found in the year 1784, at the seat of his lordship the earl of Egremont, at Petworth in Sussex, in hope that this learned and inquisitive age will either think my endeavours about them worthy of its assistance, or else will be thereby induced to attempt some other means of their publication. The only undeniable proof I can give now of the usefulness of such an undertaking, is by giving a succinct report of their contents, and by shewing briefly what may be effected with these materials: And although I come to the performance of such an enterprise with much less abilities than the different parts of it require, yet I trust that my love for truth, my design and zeal to vindicate the honor due to an Englishman, the author of these manuscripts, which are the chief reasons that have influenced me in this undertaking, will serve as my excuse.

"A predecessor of the family of lord Egremont, that noble and generous earl of Northumberland, named Henry Percy, was not only a generous favourer of all good learning, but also a

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[c] See P. 17.

patron and Mæcenas of the learned men of his age. Thomas Harriot, the author of the said manuscripts, Robert Hues, and Walter Warner, all three eminent mathematicians, who were known to the earl, received from him yearly pensions; so that when the said earl was committed prisoner to the Tower of London in the year 1606, our author, Hues and Warner, were his constant companions; and were usually called the earl of Northumberland's three magi.

"Thomas Harriot is a known and celebrated mathematician amongst the learned of all nations, by his most excellent work, entitled, 'Artis Analyticæ Praxis, ad æquationes algebraicas nova expedita et generali methodo, resolvendas. Tractatus posthumus.' Lond. 1631. Dedicated to Henry earl of Northumberland; published after his death by Walter Warner. It is remarkable that the fame and the honor of this truly great man was constantly attacked by the French mathematicians; for they could not bear that Harriot should in any way diminish the fame of their Vieta and Descartes, especially the latter, who is openly accused of plagiarism from our author. [See Montucla's Histoire des Mathématiques, part III. pag. 485 & seq.—Lettres de Mr. Descartes, tome III. pag. 457. Ed. Paris, 1667. 4to.—Dict. de Moreri, word "Harriot."—Encyclopedie, word "Algébre."—Lettres de M. de Voltaire, sur la nation Angloise, lettre 14.—Mémoire de l'abbé de Gua, dans les Mém. de l'Acad. des sciences des Paris pour 1741.—Jer. Collier's great historical dictionary, word 'Harriot.'—Dr. Wallis's preface to his Algebra[d].] Descartes published his Geometry six years after Harriot's work appeared, viz. in the year 1637; sir Chas. Cavendish, then ambassador at the French court at Paris, when Descartes's Geometry made its first appearance in publick, mentioned it to the famous geometrician Roberval, that these improvements in Analysis have been already made these six years in England, and shewed him in consequence Harriot's Artis Analyticæ Praxis, which, as Roberval was looking over at every page he cried out, 'Oui! Oui! il l'a vu!' 'Yes! Yes! he has seen it!' Cartesius had also been in England before Harriot's death, and had heard of his new improvements and inventions in Analysis. A critical life of this man, which his papers would enable me to publish, will shew more clearly what to think upon this matter, which I hope may be discussed to the due honor of our author.

"Now all this belongs to Harriot the celebrated analyst; but it has not hitherto been known that Harriot was an eminent astronomer, both theoretical and practical, which first appears by these manuscripts; amongst which, the most remarkable are 199 observations of the Sun's spots[e], with their drawings, calculations, and determinations of the sun's revolution round its axis. There is the greatest probability of Harriot's being the first discoverer of these spots before Galileo Galilei, or Scheiner[f]. The earliest intelligence we have of the first discovered solar spots, are of one Joh. Fabricius Phrysius, who in the year 1611 published at Wittemberg a little treatise, entitled, 'De Maculis in sole observatis et apparente eorum cum sole conversione narratio.' Galilei, who generally is taken for the first discoverer of the solar spots, published his book, 'Istoria e dimonstrazioni intorne alle Machie solare e loro accidenti,' in Rome in the year 1613. His first observation in this work is dated June the 2d, 1612. Angelo de filiis, the editor of Galilei's work, who wrote the dedication and preface to it, mentions, pag. 3. that Galilei had not only discovered these spots in the month of April in the year 1611, in Rome, in the Quirinal garden, but had shewn them several months before (molti mesi innanzi) to his friends in Florence: And that the observations of the disguised Apelles [he calls himself Apelles post tabulam] (the jesuit Scheiner, a pretender to this first discovery) were not later[g] than the month of October in the same year, by which the epoch of this discovery was put to the beginning of the year 1611. But a passage in the first letter of Galilei's works[h], pag. 11. gives a more precise term to this discovery. There Galilei says, in plain terms, that he had observed the spots in the sun before these 18 months. The date of this letter is of May the 14th, 1612, which brings the true epoch of this discovery to the month of November 1610. But, however, Galilei's first produced observations are only from June the 2d, 1612, and those of father Scheiner of the month of October, in the same year[i]. But now it appears from Harriot's manuscripts, that his first observations of these spots are of December the 8th, 1610. It is not likely that Harriot could have this notice from Galilei, for I find this mathematician's name[j] never quoted in Harriot's papers. I find him mentioning Josephus a Costa's book I. chap. II.[k] of his Natural and Moral History of the West Indies, in which he relates that in Peru there are spots to be seen in the sun which are not to be seen in Europe. It rather seems that Harriot had taken the hint from thence; besides, it is very likely that Harriot, who lived with such a generous patron to all good learning and improvements, had got the new invention of telescopes in Holland[l] much sooner in England than they could reach Galilei, who at that time lived in Venice. Harriot's very careful and exact observations of these spots, shew also that he was in possession of the best and most improved telescopes of that time; for it appears he had some with magnifying powers of 10, 20, and 30 times[m]. At least there are no earlier observations of the solar spots extant than his: they run from December the 8th 1610[n], till January 18, 1613. I compared the corresponding ones with these observed by Galilei, and found betwixt them an exact agreement[o]. Had Harriot had any notion about Galilei's discoveries, he certainly would have also known something about the phases of Venus and Mercury[p]; especially about the singular shape of Saturn, first discovered by Galilei; but I find not a word in all his papers about the particular figure of that planet.

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[d] The reference should rather be to chap. LIII. of the Algebra, especially in the Latin translation, which has many additions to what is contained in the English.

[e] See P. 32.

[f] See P. 37.

[g] "earlier."

"Of Jupiter's satellites—I found amongst his papers a great set of observations, with their drawing, position, and calculations of their revolutions and periods: his first[q] observation of those discovered satellites I find to be of January the 16th, 1610, and they go till February the 26th, 1612. Galilei pretends to have discovered them January the 7th, 1610: there is then all probability of Harriot's being likewise the first discoverer of these attendants of Jupiter[r].

"Amongst his other observations of the moon, of some eclipses[s], of the planet Mars[t], of solstices[u], of refraction[x], of the declination of the needle[y], there are most remarkable ones of the famous comets of 1607 and of 1618,[z] the later, for there were two this year, [Kepler de Cometis, p. 49:] they were all observed with a cross staff, by measuring their distances to fixed stars, which makes these observations the more valuable, because they had but grossly been observed; Kepler himself observed the comet of 1607, but with the naked eye[a], pointing out the place where it stood by a coarse estimation, without an instrument; and the elements of their orbits could, in defect of better observations, only be calculated by them. The observations of the comet of the year 1607, are the more of importance and consequence, even now for modern astronomy, as this is the same comet that fulfilled Dr. Halley's most wonderful prediction of its return in the year 1759. Halley's prediction was only grounded upon the elements these coarse observations of it could give him; so he only assigned the term of its return to a space of a year. The most intricate calculations of the perturbations of this comet, done afterwards by M. Clairaut, reduced these limits to a month's space. We may now throw a greater light upon this matter by the more accurate observations on this comet by Mr. Harriot. In the month of October, in the year 1785, when I conversed upon the subject of Harriot's papers, and especially upon this comet, with the celebrated and eminent geometrician M. de la Grange, director of the Royal Academy of Sciences at Berlin, he suggested to me at that time an idea, which, if brought into execution, will clear up an important point in astronomy. It is known to astronomers how difficult a matter it is to determine Saturn's mass, and how little satisfactory the notions of it are hitherto; the whole theory of perturbations of comets depending upon this uncertain datum, several attempts and trials have been made towards this exact determination by the most eminent geometricians of this age, and especially by M. de la Grange himself; but being never satisfied with the few and uncertain data by which this problem may be resolved, he thought that Mr. Harriot's observations on the comet of 1607, and the modern ones of the same comet in the year 1759, would suggest a way to resolve the problem *à posteriori;* that of determining by them the elements of its ellipsis, the retardation of the comet compared to its period, may clearly be put to the account of the attraction and perturbation he has undergone in the region of Jupiter and Saturn; and as the part Jupiter acts in that is thoroughly known, the remainder will be Saturn's share, from which the mass of the latter may be inferred. In consequence of this consideration I have already begun to reduce most of Harriot's observations of this comet in order to calculate by them the true elements of its orbit in an elliptical hypothesis, to complete M. de la Grange's idea upon this matter.

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[h] "Work:" the page referred to is that of the Istoria.

[i] "The preceding year."

[j] See P. 29, 36.

[k] See P. 37. No paper of Harriot's has been found with this reference on it.

[l] See P. 20. [m] See P. 20, 28, 34. [n] See P. 37.

[o] See P. 32.

[p] Galileo was never able to see the phases of Mercury. Opere, tom. IV. P. 244.

[q] See P. 29. [r] See P. 29. [s] See P. 21.

[t] See P. 40. [u] See P. 40. [x] See P. 40.

[y] See P. 41. [z] See P. 19.

[a] There were no telescopes in 1607; but Kepler's sight was not good, and he sometimes used a lens to assist it.

"I do not mention here more of Harriot's analytical papers, which I found in a very great number; they contain partly several elegant solutions of quadratic, cubic, and biquadratic equations, partly other solutions, and loca geometrica, which manifest his eminent attainments, and will serve to vindicate them against the attacks of several French writers, who refuse him the justice due to his skill and eminences, merely to save Descartes's honor, who yet, by some impartial men of his own nation, was accused of publick plagiarism.

"Thomas Harriot was born at Oxford in the year 1560. After he had been instructed in grammar learning, be became a batteler or commoner at St. Mary's Hall: he took the degree of bachelor of arts in 1579. Soon after he came to the knowledge of the heroic knight sir Walter Raleigh, who allowed him a yearly pension. In 1584 he went with the said knight, and first colony, into Virginia, where he was employed in the discovery and surveying thereof; maps[b] of which I have found very neatly done amongst his papers. After his return he published 'A brief and true report of the Newfoundland of Virginia, of the commodities there found to be raised, &c. London, 1588.' It was put into Latin, and printed at Frankfort in the year 1590. Sir Walter Raleigh introduced him into the acquaintance of the earl of Northumberland, who did allow him a yearly pension of 300*l*. Wood, in his Athen. Oxonien. mentions only 120*l*. but by some of his receipts[c], I found amongst his papers, it appears he had 300*l*. which indeed was a very large sum at that time. Wood, in his Athen. Oxon., mentions nothing of Harriot's manuscripts; he only tells us of a manuscript in the library at Sion college, London, entitled, Epheremis Chyrometrica[d]. I got an access to this library and manuscripts, and was indeed in hopes of finding something more of Harriot's; for most of his observations are dated from Sion college[e]; but I could find nothing from Harriot himself. I found some other papers of his friends; he mentions in his observations one Mr. Standish, at Oxford, and Nicol. Torperly[f], who also was of the acquaintance of the earl of Northumberland, and had a yearly pension; from the former I found two observations of the same comet of year 1618[g], made at Oxford, which he communicated to Mr. Harriot. Thomas Harriot died the 2nd of July, 1621. His disease was a cancerous ulcer in the lip, which some pretend he got by having had the custom of holding the mathematical brass instruments, when working, in his mouth. I found several letters of his, and answers to them, from his physician Dr. Alexander Rhead, who in his treatise mentions Harriot's disease. His body was conveyed to St. Christopher's church, in London. Over his grave was soon after erected a monument, with a large inscription thereon, but destroyed with the church itself by the dreadful fire of Sept. 1666. He was but 60 years of age. [See Wood's Athen. Oxonien. word 'Harriot,' p. 390, 391, 392."]

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[b] These maps were searched for in 1820, when only two were to be met with, one of the American hemisphere, the other of North America only. The following is the account drawn up at the time by the gentleman who made the inquiry. "These maps are on " vellum, neither of them finished, nor on such a scale " as to deserve the name of a survey, nor indeed to be of " any use in determining the precise form of a coast, as " will be understood when I say, that though the names " are crowded together, and written in a very minute " character, no more than twenty names are inserted " from C. Florida to the Chesapeak." Indeed Wood was mistaken in the view which he took of Harriot's mission. Oldys describes him as "that virtuous and " religious man, who, being sent over by sir Walter " Ralegh with his first colony to Virginia, but did not " bear him company, as the said author (Wood) mistakes, to survey the bounds of the English discoveries, " with the commodities and customs of the country, did " first communicate the gospel to those heathen inhabitants." (Life of Sir. W. Ralegh, p. 177. Oxf. Ed.)

NOTE D, p. 18.

The following are copies of Dr. Robertson's two reports[h].

"October the 20th, 1794.

"The following are the titles of the papers put into my hands in July last, in order to be examined:

1. Περὶ χωρίου ἀποτομῆς: seu de spatii resectione: Propositio generalis: ex lib. 7. Pappi.
2. De centro gravitatis pyramidis.
3. Ptolomaicum elementum de compositione rationum.
4. Theoremata ad subtensas periferiarum.
5. Lemmata.
6. Problemata.
7. De Parabola.

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[c] These documents have not been met with. Probably there is some mistake: we could hardly expect to find the acquittances among the papers of the individual who received the money.

[d] See P. 40.

[e] See P. 22.

[f] Torporley's name was Nathaniel, but by some mistake, possibly of the printer's, he is also in one place called Nich. (Wood Ath. Ox. vol. II. p. 300.) The university registers having been consulted to ascertain which was right, another correction was also found necessary. Wood says (p. 524) that after having been with Vieta, "he returned to the University, and "entring himself into Brazennose Coll. did as a member thereof take the degree of master of arts, an. "1591." Now there are no traces in the college books of his having belonged to B.N.C. but it happened that the person was of that society whose name stands immediately before Torporley's in the register for degrees, and Wood's eye, by passing from one to the other, most probably occasioned the mistake. Torporley certainly became M.A. as a member of Christ Church, where he originally entered. See also note [b], P. 53.

[g] "1607."

[h] The object of these reports has been mistaken. Dr. Robertson was merely applied to for an opinion on the expediency of printing the whole or any part of Harriot's papers, and he confined himself in his answer to this specific point. It may be seen, for example, that he mentions Oct. 17, 1610, as the first day on which Harriot saw a satellite of Jupiter, but he takes no notice of the B. de Zach having spoken of an observation in the preceding month of January. In a communication of this kind Dr. Robertson kept to the inquiry which was submitted to him, and did not embarrass the question by any unnecessary historical disquisitions. It ought possibly to be explained that the long interval between the reports was occasioned by the second portion of the papers having originally been put into the hands of another gentleman, who was prevented from attending to them.

8. De centro gravitatis trianguli.
9. De centro gravitatis parabolæ.
10. De Asymptotis.
11. De reflectione corporum rotundarum.

"These papers, excepting the last, are in no point of view fit for publication. The greatest part of them consist of detached and unfinished explanations of the authors which he read; begun, according to all appearance, with the design of satisfying his own mind upon the subject before him, and dropped abruptly as soon as this satisfaction was obtained.

"The 1st, 2nd, 3rd, 4th, 5th, and 6th, of the abovementioned articles are of this kind. The 7th, 8th, 9th, and 10th articles, seem to have been entered upon with an intention of treating the subjects in a more perspicuous way than any which had been pursued before his time; and had he written the 7th and 10th with a view to publication, there is every reason to suppose that in these two he would have succeeded in his design. In point of matter, as far as they extend, they are ingenious improvements upon Apollonius; but the same improvements, fully and elegantly demonstrated, are to be found in Mydorgius's Conic Sections, published in 1631. I should suppose that Harriot had not read Archimedes when he wrote the 8th and 9th articles, as they are so very much inferior to what we have by that celebrated mathematician on the subject.

"To these remarks on the substance of the first ten articles, it may be proper to add, that they are destitute of the principal particulars requisite in regular mathematical composition. No first principles are laid down; due arrangement is overlooked; and the demonstrations, often defective, are expressed in a kind of algebraical shorthand. In saying this, I by no means intend to insinuate any thing disrespectful to the memory of Harriot. I offer these observations as reasons for my firm persuasion that he never intended the papers for publication; and that it would be injurious to his reputation to print them.

"The paper 'de reflectione corporum rotundorum,' when compared with those already mentioned, may be considered as highly finished, but to its publication some strong objections may be made. Harriot himself states its imperfections in his letter, which accompanies this; and to these imperfections it may be added, that every thing depending upon the composition and resolution of forces is so much better understood, and more clearly treated, since the discoveries of sir Isaac Newton, that it would suffer much upon a comparison with modern publications. The subject is more fully and more elegantly handled in Keill's Introduction to Natural Philosophy."

"Feb. 15, 1798.

"To a great many of these papers there is no title, nor do they admit of any specific description. They appear to be rough calculations of some particulars which he wished to ascertain, without any allusion to the data with which he set out, or obvious tendency to the object in view. Others admit of classification, and afford abundant proof of Harriot's zeal in the cause of science, and of his unremitting attention to its improvement.

"The first class of this description relates to the spots on the sun. From these papers it appears that he first began to observe the spots on the 8th of December in 1610, and that he continued to observe them, at irregular intervals, to the 18th of January 1613. The observations recorded are 199 in number, and the accounts of them are accompanied with rough drawings, representing the number, position, and magnitude of the spots. From the manner, however, in which these observations appear to have been made, and also from that in which I find them recorded, I do not think that Harriot ever intended them for publication; nor do I think that the publication of them now would either satisfy rational curiosity, or contribute, in the smallest degree, to the advancement of astronomy. The circumstances under which the observations were made are very briefly and very vaguely recorded, and consequently, no calculations can be founded upon them likely to lead to accurate conclusions. These deficiencies I am inclined to attribute, partly to his having had no intention to publish on the subject, and partly to the imperfection of astronomical and philosophical instruments in his time.

"From this class of papers of which I am now speaking, it plainly appears that Harriot had no coloured glass to defend the eye, for the following expressions frequently occur, 'A mist,'—'a notable mist,'—'misty and cloudy,'—'the sonne was somewhat to cleare. There being no cloudes but only thick ayer,'—'convenient thin cloudes,' &c.

"As there is no reason to doubt of his diligence, I think that such expressions as the following are to be attributed to the imperfections of his instruments, and not to his want of care: 'The altitude of the sonne being 7 or 8 degrees.'—'the sonne being 3 or 4 degrees hy,' &c.

"The next class of papers (fixed together with a pin) which met my eye, is entitled, 'Descriptio parabolæ per circularem motum.' This I proceeded to examine, with sanguine hopes of finding a practical method of describing a parabola by an uninterrupted motion. My hopes, however, quickly vanished; for this class only contains diagrams, so rough and confused, that it does not appear from them upon what property of the circle he founded his attempt.

"After this disappointment I proceeded to examine a class of papers entitled, 'De Jovialibus Planetis.' From two pages of these papers, it appears that he first observed Jupiter's satellites on the 17th of October, 1610, for both of them have this date; and at the top of one of them there is this expression, 'My first observation of the new planets;' and at the top of the other, 'My first observation and others following of the new found planets about Jupiter.' At the top of another page there is this expression, 'The second years observations, being anno 1611, of the Joviall Planets;' and on this same page, there are the following dates prefixed to his accounts of observations, 'Syon Octob. 1'—'Octob. 6'—'Dec. 11'—'Syon $\frac{1611}{1612}$ January 12'—'January 13'—'January 26'—'February 15'—'February 17'—'Feb. 26.'

"Rough drawings of the positions of the satellites, and rough calculations of their periodical revolutions, accompany the brief statements of the observations; but, in my opinion, astronomy could not be advanced by the publication of any part of them.

"The other papers which admit of classification, may be entitled, Observations of the Moon—Observations of the comets of 1607 and 1618,—An Examination of Willebrord Snell's observations for determining the length of a degree of a meridian,—'The effects of the Decree of the councell of Neece for the observation of Easter-day,'—'The doctrine of Nautical Triangles'—Remarks on Eratosthenes, Tycho, Kepler, Paul Hainzelus, and Vieta.

"It is needless to enter into a minute description of these papers, as they are not drawn up with any degree of regularity and precision, and as it evidently appears that they never were intended for publication.

"Upon the whole it is my opinion that the publication of the papers mentioned in this report could only tend to prove that Harriot was very assiduous in his mathematical studies, and in his observations of the heavenly bodies; it could not contribute to the advancement of science."

NOTE E, p. 19.

Dr. Hutton's addition is as follows:

"As to his (Harriot's) manuscripts lately discovered by Dr. Zach, as above mentioned, it is with pleasure I can announce, that they are in a fair train to be published: they have been presented to the University of Oxford, on condition of their printing them; with a view to which, they have been lately put into the hands of an ingenious member of that learned body, to arrange and prepare them for the press."

NOTE F, p. 37.

"... I say moreover, to confirme this truth, that the heavens move and in them the starres march in turning, the which we cannot easily discerne with our eyes, seeing we see that not onely the starres do move, but also the regions & whole parts of heaven: I speake not onely of the shining & most resplendent parts, as that which we call Via Lactea, and the vulgar, S Jaques way, but also of the darker & obscurer parts in heaven. For there we see really, as it were spots and darkenes, which are most apparent: the which I remember not to have seene at any time in Europe, but at Peru, and in this other hemisphere I have often seene them very apparent. These spots are in colour and forme like unto the Eclips of the Moone, and are like unto it in blacknes and darkenes: they march, fixed to the same starres, alwaies of one forme and bignes, as we have noted by infallible observation."

The Naturall & Morall Historie of the East and
West Indies by the R. F. Joseph Acosta
4to. London 1604. Booke I. Chap. 2. p. 8.

NOTE G, p. 30.

From the regular series of entries it does not appear that Harriot observed the satellites of Jupiter in any year on the 16th of January. It was necessary therefore to look to the loose papers which contain his calculations. On one of these the following passage occurs:

" De planetis circa ♃. pro periodo. a.
1610

from the 17th of Jan.	to the 19th Feb.	33^d	2 periods	occid.
24	26	33	2 per.	or.
15	17	33		

---

7 17	Ja.	26	Dec.	353	21 per.	occ.
16		1611		$16\frac{1}{3}$	1 per.	
26	Ja.	3	Jan.	351	21 per.	or.
				$16\frac{8}{21}$	1 per.[i]	

The Baron could hardly have derived his date from this paper. The observation which it contains was evidently made on the 26th of January, in some place where the reformation of the calendar had already been adopted, and 16 was only written over the other numbers as a reduction to the old style. Likewise if he had disregarded this circumstance, he would certainly, in taking his earliest date from this place, have assumed it to be the 7th of January, which stands in a line immediately above the other. We may therefore safely conclude that the cause of the error must be looked for in some other place, and the 16th of January, 1610, occurs nowhere else but under the following circumstances. On another paper we find:

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[i] The periods $16\frac{1}{3}$ and $16\frac{8}{21}$ are the results of the intervals having been taken originally as 343 and 344 days, for which the other numbers 353 and 351 were afterwards substituted. But 353 is the interval between the 7 of Jan. and the 26th of December, which shews that the latter observation was made in England, and the former on the continent.

De Jove primo

1610
Feb. 11^{o}. stilo novo

	or.	☌ perig.	occ.	☌ apogæa.
Feb.	11^{d} 11^{h},86	11^{d} 17^{h},17	11^{d} 22^{h},48	12^{d} 3^{h},79
	21, 24	21, 24	21, 24	21, 24
	10 14, 62	10 19, 93	11 1, 24	11 6, 55
	9 17, 38	9 22, 69	10 4, 00	10 9, 31
	8 10, 14	9 1, 45	9 6, 76	9 12, 07

In this manner the numbers are continued on a half sheet of foolscap, till at the bottom we arrive at the day in question.

[Jan.]	17 17, 14	17 22, 45	18 3, 76	18 9, 07
	16 19, 90	17 1, 21	17 6, 52	17 11, 83.

There is another paper on which this last line is copied out at the top, but the numbers on it are then carried back to the 7th of Jan. 1610. There remains therefore but one supposition on which to rest—the numbers from which the above extract has been made, most probably caught the Baron's eye; and hastily running down the column of dates, he must have taken out the last as the earliest record of Harriot's having seen a satellite of Jupiter. This is the only means which the papers afford of accounting for his assertion.

The memorandum of "stilo novo" might be expected to have suggested that the numbers did not belong to observations, which Harriot would have made in England; and it is absolutely impossible that any one could have continued to see, in such an uninterrupted series, the conjunctions and elongations of the satellite. Even if this had been possible, his observations would not have all differed from each other by intervals which were identical to the second place of decimals; and it may be seen that the quantities in each line have a common difference of 5^{h},31, while they uniformly differ from those, which are next above or below them in the same column, by 21^{h},24. All this, however, admits of a clear explanation from the real origin of the numbers. They are continued on a third paper to the 2nd of March; and prefixed to this continuation we find the following memorandum:

De Jove primo

1610
Stilo novo Feb. 11^{d} 8^{h}, 3′
vel 11 8, 05
−1, 50
11 6, 55 ☌ apogæa

In the margin there is a drawing like that in Fig. 3, Plate IV. Sup. and by the side is written: "motus a centro fuit 1^{h},50."

Galileo informs us in his Sidereus Nuncius, that in Feb. 1610, "Die 11 . . . . hora "tertia stella quarta Jovi proxima ab oriente visa est, reliquis minor, a Jove dissita per "min. 0. sec. 30." This will agree with the 30″ which is written under the mark for the satellite, but the time of the observation is set down by Harriot to be $8^h,3'$, which requires some further explanation. Galileo speaks of "hora prima, tertia, &c.—noctis," and, after the custom of his country, he reckoned not from noon or midnight, but from sunset[h]. Harriot seems commonly to have taken his data from the tables of Maginus; if therefore we make use of his Ephemerides [4to Franc. 1610.] for the obliquity of the ecliptic, the sun's longitude and the latitude of the place, the sun will be found to have set at Venice on the day in question at $5^h, 1'$ after noon, which, added to the 3 hours mentioned by Galileo, make up $8^h, 1'$. No allowance is here made for refraction, for the equation of time, or the sun's motion during the interval from noon; these quantities were probably not attended to in Harriot's calculation, but the result is sufficiently near to identify the observation: $1^h, 50$ was subtracted for the time which is supposed to have elapsed before the satellite was seen after the conjunction, and $6^h,55'$, which then remained, were assumed to be the epoch of "☌ apog." for the 11th of February. This is the quantity in the foregoing extracts from which the whole computation appears to have been derived.

Again, on another paper we find as follows:

De periodo Jovium primi.

	1610 stilo novo				
A	Jan. 26^d	9^h	$40'$		
	15	22	24	$382^h\frac{40}{100} = 18$ period.	
B	Feb. 11	8	4		ut proxime
	15	22	27	$382\ \frac{45}{100} = 18$ period.	
C	Feb. 27	6	31		
	31	20	51	764, 85 = 36 period.	

bg 3′½, *bf* 1′ 30″, ergo *xe* $25^\circ\frac{38}{100}$

A ♃ 10°54′ ♊ R	B ♃ 10°51 ♊ Dir.
hoc est 10,90 ♊ *zab*	hoc est 2^s 10, 85 ♊
+ 25,38	+ 25,38
A (^{1}♃) 3^s6,28 ♋ *ebd*	B (^{1}♃)3 6,23 ♋
C ♃ 11°36′ ♊ Dir.	$\left.\begin{matrix}360\\18\end{matrix}\right\} = 6480^\circ$
hoc est 2^s 11,60 ♊	
+ 25,38	$\left.\begin{matrix}360\\36\end{matrix}\right\} = 12960^\circ.$
C (^{1}♃) 3^s6,98 ♋	

A. B.

$$6479^\circ,95 : \underbrace{382^h,4000}_{15^d\ 22^h\ 24'} :: 360^\circ,00 : 21^h,2446$$

A. C.

$$12960,70 : \underbrace{764^h,8500}_{31^d\ 20^h\ 51'} :: 360^\circ,00\ .\ 21,2447$$

By the side is a drawing like that in Fig. 3. Pl. IV. Sup., and against 30″ is written; "hoc est 1′½ a centro Jovis."

The smaller letters (*b*, *g*, &c.) refer to Fig. 4. Pl. IV.

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[h] Lalande, Voyage en Italie, Pref. xxxii.

The longitudes of Jupiter are taken from the Ephemerides of Maginus, and Harriot assumes $3'\frac{1}{2}$ as the greatest apparent distance of the first satellite from the centre of the planet: from these data he calculates that its revolution is performed in $21^h,24$. His method of computation is sufficiently clear, but it may be useful to add some remarks on the particulars to which he applies it. B is evidently the same observation of Galileo, which has just been discussed; it is now set down 1′ later, but if the rough manner is considered in which the time has been found, this will not be material. C is likewise from the Sidereus Nuncius, where it is said in 1610, Feb. "die 27 hora 1 min. 4 ..... "orientalior a Jove min. 10, sequens Jovi proxima min. 0 sec. 30." Calculating again with the numbers found in Maginus, the sun set at Venice on this day at $5^h,25'$ after noon; to which if $1^h,4'$ be added, the sum will be $6^h,29'$, or only 2′ less than the time given by Harriot. A occurs also in the same work, where it is said, Jan. 1610, "die "26 . . . hora quinta . . prope Jovem quarta stellula ex oriente emergebat cæteris minor, a Jove tunc remota m. 30." The distance is obviously wrong in its designation. It most probably was intended, like the others, to have been written "min. 0. sec. 30," from which the middle has been omitted by some mistake [i]. It is clear that Harriot at least understood it so, (which is sufficient for the present purpose,) for he assumes the relative position of the satellite to be the same in each of the three observations, and consequently makes no allowance in any of them for the time which might have elapsed from the central conjunction. Now the sunset at Venice for this day comes out $4^h,40'$ from noon, which added to 5^h, make the time of observation $9^h,40'$, exactly as it is here set down.

There is a separate paper, on which Harriot has divided the interval of 382,4 days by a series of numbers from 16 to 25. No reason is given for fixing on them, and they led him into a mistake, which he might have avoided by the trial of some smaller divisors. Of all the periods which he arrived at in this manner there was none which could agree with the time of the other conjunctions, excepting that which supposed the intervening repetitions to be 18. This being a multiple of the real number, might answer to all which were observed, and only made it necessary to imagine that others had occurred without having been seen. By this means, however, the duration of the revolution was reduced to one half of what it ought to be, and a fundamental error was in consequence introduced into the table which contains the supposed observation of the 16th of Jan. 1610. It follows also that all the quantities in the first and third columns are wrong, and that for 1610, Jan. "$16^d,19^h.90$" could not even be supposed to represent the time of either the satellite's conjunction or elongation.

The first and the last observations of the satellites which Galileo gives in the Sidereus Nuncius are on the 7th of January and the 2nd of March, 1610, which are precisely the days on which Harriot's table begins and ends. It is clear, therefore, although we find calculations "ex propriis observationibus," that in this case he depended rather on Galileo's observations than on his own—or the table might have been constructed before he began himself to observe.

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[i] The error occurs in the first edition, and has been copied in all the rest.

NOTE H. p. 42.

See note D.—Dr. Robertson has thrown his two reports together in his communication to sir David Brewster, and he prefixed the following reason for publishing them. The Baron de Zach says, with reference to this publication, “ nous ignorons comment et pourquoi après trente-huit ans de repos, on est revenu sur ces manuscrits;” but the cause existed in the misapprehensions which had been occasioned by his accounts of the papers, and which would not suffer the question to remain at rest.

In No. LI. art. 2. of the Edinburgh Review, when speaking of Baron de Zach, it is said, “ Several years ago he visited England, and resided there for a considerable time. He lived much in the family of lord Egremont; and we owe to him the discovery of several unpublished MSS. of Harriot, one of the ablest and most inventive mathematicians of the age in which he lived. These the Baron found among the papers of the nobleman just named. They have since been consigned to the care of the University of Oxford; and are now, we have no doubt, in the progress toward publication.”

The belief implied in the conclusion of what has been quoted, ought not to have been entertained; for the manuscripts in question had been examined in Oxford, had been declared to be unfit for publication, and had been returned to the nobleman to whom they belonged more than sixteen years before the Edinburgh Review had arrived at No. LI.

NOTE I. p. 26.

The Lowers were a very old and respectable family in Cornwall, which in 1620 could trace up a pedigree[k] of thirteen generations. Carew records in his Survey of Cornwall, that Thomas was in 1602 settled at St. Winnow, between Lostwithiel and Fowey; and from an epitaph in the church of Landulph[l], it appears that his eldest son, sir William Lower, knt. died in Carmarthenshire. Gilbert[m] mentions, that sir William “ served in parliament for the borough of Lostwithiel in the 1st of James the Ist;” and this is nearly all the authentic information which I can find collected about him.

The parish registers of St. Winnow unfortunately do not go back far enough to give us any information with respect to the time of his birth; but it appears from the Oxford Matriculation books that William Lower, the son of a gentleman from Cornwall, entered at Exeter College June 10, 1586, and that he was then sixteen years of age. The college books for this year are not to be found, but the name appears subsequently till the summer of 1593, when it was removed without any indication of the individual's having taken a degree. We have indeed no direct evidence from which it can be certain that this is the same person, but it is not improbable; for if sir William was born about

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[k] Lysons's Magna Britannia, Cornwall, p. cxl.

[l] “ Here lyeth buried the body of sir Nicholas Lower of Clifton, Knight, descended of the house of St. Winnowe, the sonne of Thomas Lower and Jane his wife, one of the coheyres of Reskymer, who had issue six sonns, viz. Sir William Lower Knight, deceased in Carmarthenshire, John Lower, the said sir Nicholas Lower, sir Francis Lower Knight, Thomas Lower, deceased in London, and Alexander Lower. He married with Elizabeth, one of the daughters of sir Henry Killigrew of London Knight, and dyed without issue, surrendering his soule to his Redeemer at Clifton the 17th day of Maye A. D. 1655.”

[m] Historical Survey of Cornwall, vol. II. p. 192.

1570, he would have been of the very suitable age of thirty-one, when he was returned in 1601 as member for Bodmin to the last parliament of Queen Elizabeth. It has been mentioned that at the accession of James the First he was returned for Lostwithiel. He was knighted in the latter end of March 1604; this is clear from the Journals of the House of Commons, for he is called on the 26th Mr., and on the 28th sir William Lower [n]. King James did not dissolve the parliament till 1614, so that he was a member during the time that we have notices of his correspondence and intercourse with Harriot.

In 1607[o], in passing over from Ilfracombe to Carmarthenshire, he saw the comet, of which he sent the observations to Harriot. It is highly probable that he was then married, for he speaks of observing with the cross-staff that he "sent down for measuring of "land;" and it does not appear that the family had any property in Wales, excepting what came to them by the marriage of sir William with Penelope, the heiress of the Perrots. By this connexion he became possessed of a fine estate of 3000 acres, near Llanfihangel Abercowen, and one of the farms on it still retains the name of Trefenti, or Treventi [p], which is evidently the same place from which sir William dates his letters. There is also another of the farms, called Lower Court, which is said upon traditional authority to have been inhabited about 150 years ago by a person of the name of Thomas Lower.

Sir William was familiar with Latin, for he frequently uses it. Besides those which have been selected upon the present occasion for publication, there are several of his letters in the British Museum, and one of them is written to Harriot in that language. There can be no doubt of his attachment to science, and we find him interesting himself in all Harriot's pursuits. It appears likewise that the "Traventane philosophers[q]" had observed the moon before the discoveries of Galileo were known in this country. It was no small effort in those days for a country gentleman to study Kepler, and the sound judgment with which he seized what was really valuable in that extraordinary writer is worthy of remark. "I cannot phansie," he says, "those magneticall natures[r];" but he catches the true spirit of Kepler's genius, admires, as they deserve, the great laws which he elicited, and reasons from them in a truly philosophical manner, when he proposes to extend the elliptical theory from the planets to the comets.

Whitlock [s] mentions that when Goring, with the king's troops, attacked Christ Church in 1644, he was beat off by major Lowre. Gilbert [t] supposes this to have been the sir William Lower of whom we are now speaking, and says, "it appears pretty certain that "he afterwards engaged in the king's service;" but even if he was alive at the time, (of which there may be some reasonable doubts,) it is not likely that when turned of seventy he could have taken an active part in the civil wars. The person alluded to was most probably his nephew, (a son of his brother John,) the dramatic writer of the same name;

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[n] He was called Lowre in the first instance, but this variation of orthography is of no importance: nothing was more common in those days. Besides, there was no other member of the same name in that parliament, and on the 2d of April we find him again called sir William Lowre.

[o] Bradley's Misc. Works, p. 512.

[p] At p. 514. of Bradley's Misc. Works the name is printed Irakenti, but a further examination by more experienced eyes clearly made out that the first letter was T, not I, and the fourth (which in form is not unlike a *x*) was either v or u.

[q] P. 25.

[r] P. 43.

[s] Memorials, p. 119.

[t] Survey of Cornwall, vol. II. p. 192.

for although Wood[u] vaunts his loyalty, it is clear from the Journals of the House of Commons that he was made governor of Winchester castle in 1645, upon the recommendation of sir William Waller. He left his party, however, very soon after, and joined the king; for we find him in Jan. 1646 a prisoner at Abingdon, where Brown commanded for the parliament. How he managed to escape with his life is uncertain; but in the beginning of the following year he was permitted to go beyond seas, upon giving security not to act any thing against the parliament[x]. This leaves little doubt about the individual, for it is known that the younger sir William Lower lived for many years, previous to the restoration, in Holland.

Sir William, in his letter of the 6th of Feb. 1610, mentions the death of his “ onlie “ son ;” and in March 1611 he tells Harriot, “ I have lost my second boy also.” What other family he might have had is not known, but he certainly left a son and a daughter. Sir Nicholas Lower makes a bequest (in a will executed Jan. 30, 1653.) of estates and property to Thomas Lower, esq. of St. Winnow, “ son of my brother sir William “ Lower,” who appears to have been returned for East Looe to the long parliament of 1640. When Charles the First called upon the members to join him at Oxford, he obeyed the summons: he was there created Master of Arts in January 1643, and Wood in his Fasti[y] says, “ I take this to be the same, who was son and heir of sir W. Lower, “ of St. Winnow in Cornwall, knight, which Thomas dying a bachelor 5. Feb. 1660, “ was buried in the parish church of St. Clement's Dane, within the liberty of West- “ minster.” In consequence of his joining the king, the parliament expelled him in Jan. 1644: and he is possibly the individual, who, at the recommendation of sir Thomas Fairfax in Feb. 1647, with some other Cornish gentlemen, (whose interests and endeavours had been very useful in “ reducing the west,”) was allowed to compound “ at “ two years' full value, for taking off their respective delinquencies, and the sequestra- “ tion of their estates[z].”

On his death the family property appears to have been inherited by his sister, who married a brother of Drummond, the earl of Perth. Her husband was knighted by James the First, and Lady Drummond's estates were divided among her four daughters. The lands in Wales descended to the second, who married Charles Trevanion of Carhays, esq., and the fourth, who became the wife of Francis Plowden, Esq. of Shiplake, near Henley in Oxfordshire. The Trevanion family have long parted with their share, but the other moiety, in which is the farm of Treventi, has lineally passed to the present possessor of it, Edmund Plowden, esq. of Haughton Hall, near Shifnal.

[u] Ath. Ox. vol. III. p. 544.

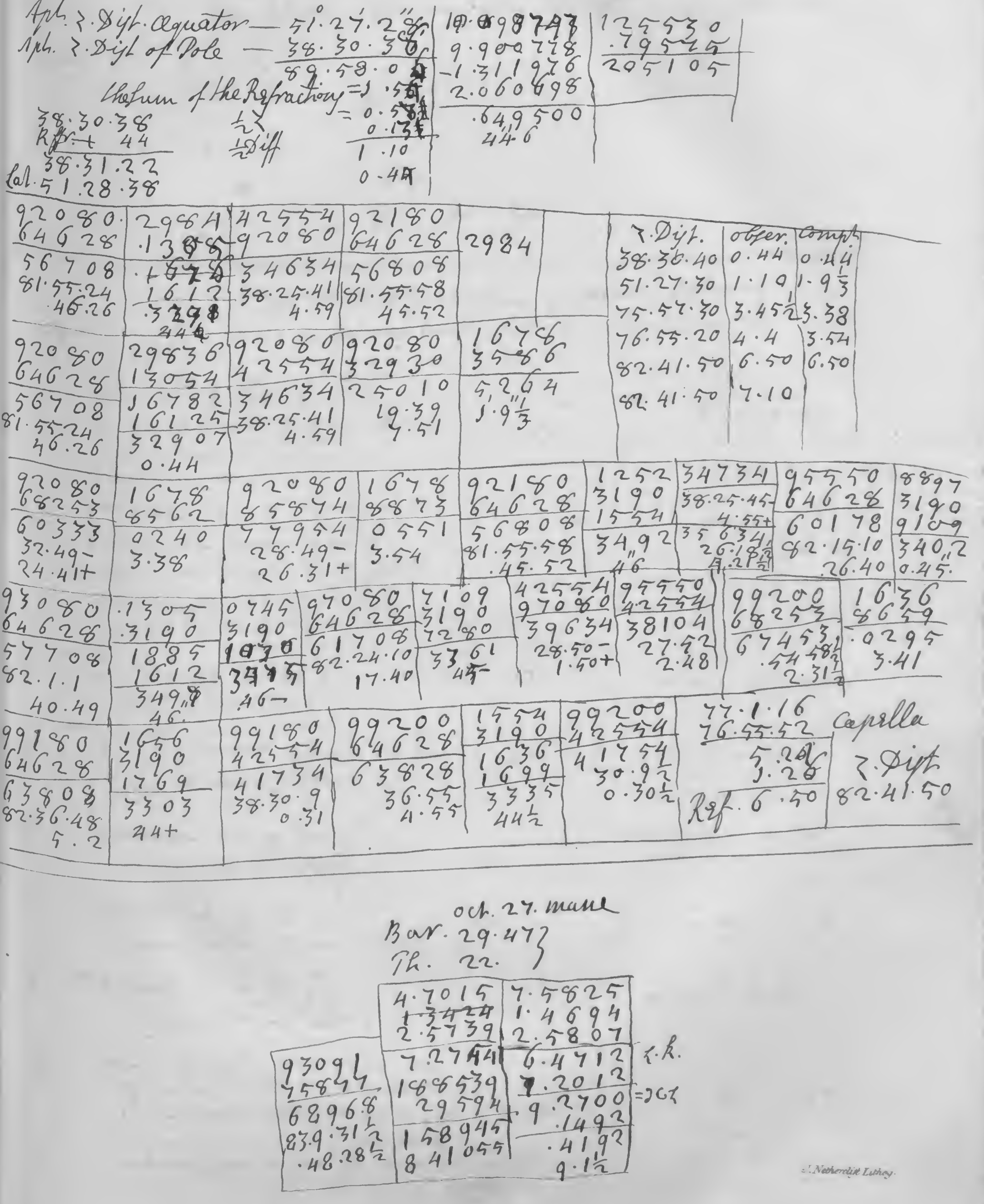
[x] Journals of the House of Commons.

[y] Vol. IV. Part. II. p. 33.

[z] Journals of the House of Commons.

CORRECTIONS.

P. 13, line 12, *for* −2.5739 *read* +2.5739
P. 20, note [o], *for* Lestwithiel *read* Lostwithiel
P. 28, l. 34, *for* Peyresc *read* Peiresc
P. 31, l. 34, *for* III. *read* IV.
P. 48, l. 20, *for* suggests *read* on this very account suggests



Aph. ζ·Dist·Æquator — 51°·27'·2"
Aph. ζ·Dist of Pole — 38·30·36
89·58·0
the sum of the Refractions = 1·5[illegible]
$\frac{1}{2}$ ζ = 0·58
$\frac{1}{2}$ Diff 0·13
1·10
0·47

38·30·38
Refr. + 44
38·31·22
Lat. 51·28·38

10·098747
9·900778
-1·311976
2·060698
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44·6

125530
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95550
42554
38104
27·52
2·48

99200
68253
67453
·54·58$\frac{1}{2}$
2·31$\frac{1}{2}$

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64628
63808
82·36·48
5·2

1656
3190
1769
3303
44 +

99180
42554
41734
38·30·9
0·31

99200
64628
63828
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4·55

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44$\frac{1}{2}$

99200
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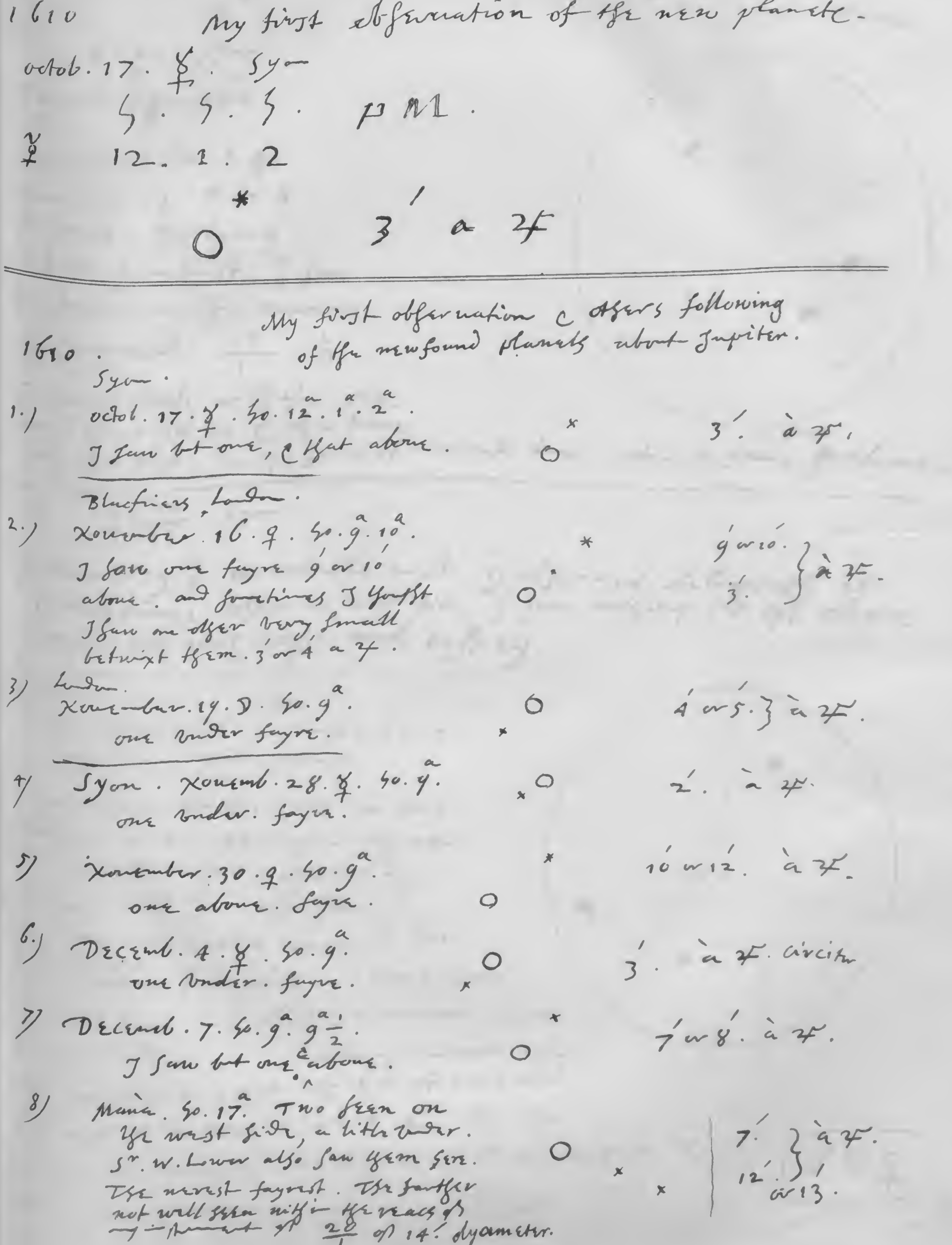


1610 My firſt obſervation of the new planete-

octob. 17. ☿. Syon

h. h. h. p M.

☿ 12. 1. 2

3′ a ♃

My firſt obſervation & others following
of the newfound planets about Jupiter.

1610.

Syon.

1.) octob. 17. ☿. ho. 12^a. 1^a. 2^a.
I ſaw but one, & that above. 3′. à ♃.

Blackfriers, London.

2.) Nouember. 16. ♀. ho. 9^a. 10^a.
I ſaw one fayre 9′ or 10′
above. and ſometimes I thought
I ſaw one other very ſmall
betwixt them. 3′ or 4′ a ♃.
9′ or 10′. / 3′. } à ♃.

3) London.
Nouember. 19. ☽. ho. 9^a.
one under fayre. 4′ or 5′. } à ♃.

4) Syon. Nouemb. 28. ☿. ho. 9^a.
one under. fayre. 2′. à ♃.

5) Nouember. 30. ♀. ho. 9^a.
one above. fayre. 10′ or 12′. à ♃.

6.) Decemb. 4. ☿. ho. 9^a.
one under. fayre. 3′. à ♃. circiter

7) Decemb. 7. ho. 9^a. $9^a\frac{1}{2}$.
I ſaw but one above. 7′ or 8′. à ♃.

8) Mane. ho. 17^a. Two ſeen on
the west ſide, a litle under.
Sr. W. Lower alſo ſaw them here.
The nereſt fayreſt. The farther
not well ſeen with the reach of
my inſtrument of $\frac{20}{1}$ of 14′ dyameter.
7′. } 12′. or 13′. } à ♃.

J Netherclift Lithog

Pl. III. Suppt.

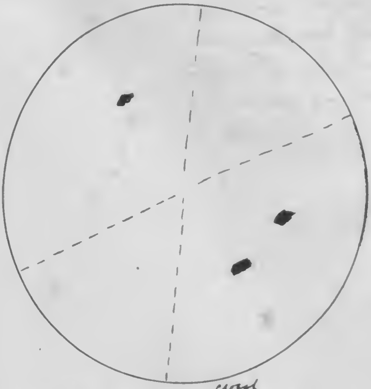
1610. Syon.

Decemb. 8 mane ♄.

The altitude of the sonne being 7 or 8 degrees. It being a frost & a mist. I saw the sonne in this manner.

Instrument. $\frac{10}{1}$. B.

I saw it twise or thrise. once with the right ey & other time with the left. In the space of a minute time. after the sonne was to cleare.



$\frac{1610}{1611}$. Syon.

January. 19. ♄. a notable mist. I observed diligently at sundry times when it was fit. I saw nothing but the cleare sonne both with right and left ey.

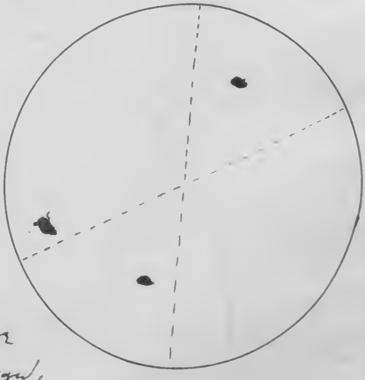
Syon.

1611. Decemb. 1. ☉. mane. ho. 10. 0′. per horologium solare.

I saw three blacke spots in such order as is here expressed as nere as I could iudge. observed $\overset{B}{\frac{10}{1}}$. Sr. w. Lower. with Chr. also saw the same. at sundry times all three I observed seen at once for halfe an houre space. at which time and all the morning before it was misty.

The greatest was that which was most oriental appearing somewhat ragged, & was of apparent angle about 2′. The other two, were neare of one bignes: & of 1′ magnitude.

eche here abouts



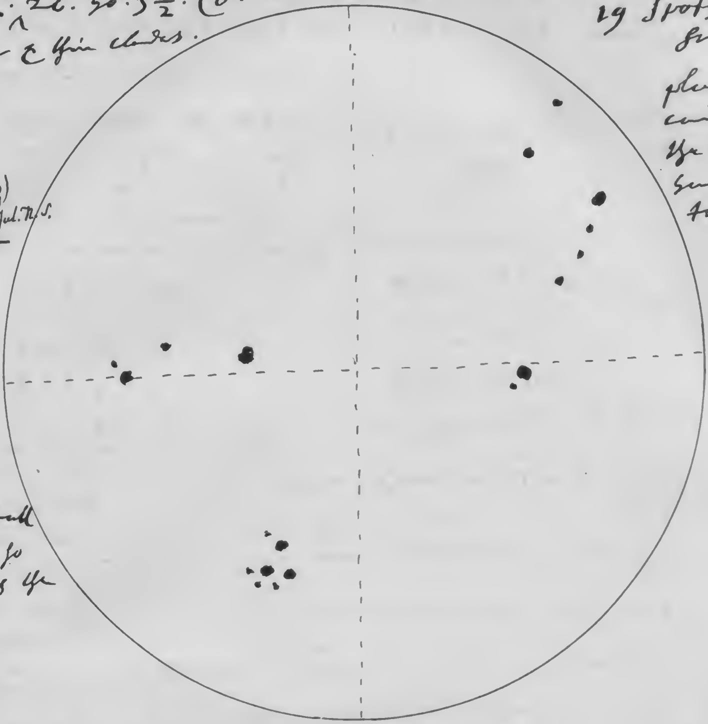
♂ ☉ ♀

J. Netherclift Lithog.

Syon. ♀ June 26. 40. 5½. (b. mane.
Thick vapor & thin cloudes.

19 Spots great & small.

places better coniectured then the day before hauing had more time.



(Galil. 90
91
3. 5 Jul. N.S.
6 —)

These 4 small spots not so black as the rest.

♃. June 11. ♃. 40. 3½. p.m.
Through thin cloudes.
11. spots.

now 5 in the cluster there. 6 more, of which the 2 southernmost placed by coniecture. hauing not time inough to examine their places but not much amisse.

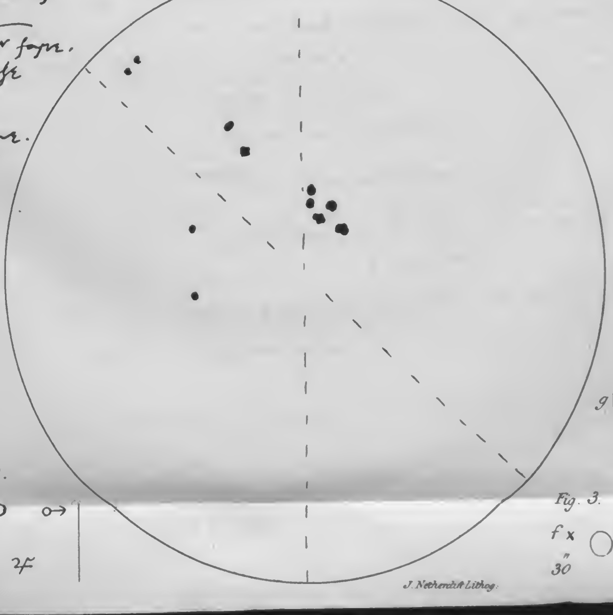


Fig. 1.

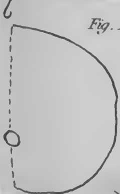


Fig. 2.

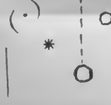


Fig. 3.

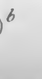
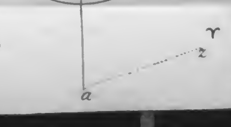


Fig. 4.



J. Netherclift Lithog.

$a = +5, -7.$ $\qquad\qquad a = +7. -5$

$aaaa - 6{,}aa + 136a = +1155 = -136{,}a - 6aa + aaaa.$

$aaaa = +6{,}aa - 136{,}a + 1155.$

$aaaa + 00{,}aa + 000a = +6aa - 136{,}a + 1155..$

$\begin{array}{cccc} ee & \frac{eeee}{4} & ee & \frac{eeee}{4} \\ = & = & = & = \\ -2. & +1. & -2 & +1. \end{array}$

Insigne exemplū
praeter sequentia.

$aaaa - 2aa + 1 = +4aa - 136a + 1156.$

1°. $aa - 1 \overset{1°}{=} +2a - 34$

$33 = +2a - aa.$

$aa - 2a = -33$

$aa - 2a + 1 = +1 - 33$

$a - 1 = \sqrt{-32}.$

$1 - a = \sqrt{-32}.$

$a = 1 + \sqrt{-32}.$

$a = 1 - \sqrt{-32}$

1°. $aa - 1 \overset{2°}{=} +34 - 2a$

$aa + 2a = 35.$

$aa + 2a + 1 = 1 + 35.$

$a + 1 = \sqrt{36}.$

$a = \sqrt{36} - 1 = +5$

$-a - 1 = \sqrt{36}$

$a = -\sqrt{36} - 1 = -7.$

2°. $1 - aa \overset{1°}{=} +2a - 34.$

$35 = +2a + aa.$

$35 + 1 = +1 + 2a + aa$

$\sqrt{36} = 1 + a$

$\sqrt{36} - 1 = +a.$

$5 = a.$

$\sqrt{36} = -1 - a.$

$-\sqrt{36} - 1 = +a.$

$-7 = a.$

° $1 - aa \overset{2°}{=} -2a + 34.$

$2a - aa = 33.$

$aa - 2a = -33.$

$aa - 2a + 1 = +1 - 33.$

$a - 1 = \sqrt{-32}$

$a = +1 + \sqrt{-32}$

$1 - a = \sqrt{-32}$

$a = +1 - \sqrt{-32}.$

$\begin{array}{l} \frac{eeee}{4} + 1155 \\ ee + 6. \end{array} = \begin{array}{c} 68 \\ 68 \end{array} = \begin{array}{c} \frac{136}{2} \\ \frac{136}{2} \end{array} = 4624.$

$\frac{eeeeee}{4} + 1155{,}ee$

$+\frac{6eeee}{4} + 6930 := 4624.$

$\frac{eeeeee}{4} + \frac{6{,}eeee}{4} + 1155{,}ee = \begin{array}{r} +4624. \\ \underbrace{-6930}_{} \\ -2306 \end{array}$

Insignis aequatio.

$-eeeeee - 6eeee - 4620{,}ee = +9224. \qquad ee = -2$

vel:

$-eee - 6ee - 4620{,}e = +9224. \qquad e = -2$

Tum

$eee - 6ee + 4620{,}e = +9224, \qquad e = +2.$

$\begin{array}{lll} vvv \;--12v & = +9224 & = +12v \\ +4620{,}v & +\quad 16 & -4620{,}v - vvv \\ & -9240 & \end{array}$

$vvv + 4608v = +0000 = -4608{,}v - vvv$

$v = +0. \qquad\qquad v = -0.$

$e = +v + 2 \qquad\qquad e = -v + 2$

$e = +0 + 2 = +2 \qquad\qquad e = -0 + 2 = +2$

vel per Canonica

$\begin{array}{lll} vv{,}vvvv - 52{,}vv & = +18496. & = +12vv \\ +4620{,}vv. & -27720 & -4620{,}vv - vv{,}vv{,}vv \\ & -\quad 16 & \\ & +9240 & \end{array}$

$vvvvvv + 4608{,}vv = +0000 = -4608{,}vv - vvvvvv$

$vv = +0$

$ee = +vv - yy.$

$ee = +0 - 2 = -2.$

J Netherclift Lith.

Chamouni 9 Août 1862.

Mon Cher Monsieur,

Comptant toujours sur la bienveillance que vous avez bien voulu me montrer, et sur le désir que vous m'aviez exprimé que je vous ferai part de ma visite au Mont Rose, je vous addresse ces peu de lignes, un peu trop tard peut-être, vû les grands détours que font les communications ~~de~~ par la poste dans ces contrées.

~~Je~~ pars demain pour le Gd. St Bernard et pour faire des courses dans les vallées entre St. B. et le Mont Rose. Je n'arriverai guère à Gressonay dans moins de 15 jours d'ici, c'est-à-dire vers le 24 Août; et je serai très reconnaissant si vous pouvez me procurer des lettres pour M. Vincent, ou pour un Baron Allemand dont j'ai oublié le nom, qui y demeurant, ou enfin pour quelque personne qui connaît les montagnes et qui voudrait bien m'aider dans les courses que je serai dans les cas d'y faire — ou me procurant des guides, ou autrement.

J'espère donc, Mon Cher Monsieur et très honorable ami, que j'aurai au moins quelques lignes de vous vers le 24, à la Poste Restante (s'il y en a) à

Gressonay- Autrement, à l'Auberge-

Je vous assure que je n'oublierai jamais la bonté que vous avez eu pour moi pendant ma visite à Turin dont je garde le plus agréable souvenir.

Depuis cela, j'ai passé 15 jours à Courmayeur ensuite j'ai passé le Col du Géant jusqu'ici, et depuis j'ai beaucoup étudié les glaciers de ces environs.

Mes Compliments très respectueux à Madame Plana- et à Madame votre fille.

Agreez Monsieur l'expression de tout mon Respect et dévouement

James D. Forbes.

P.S./ Existe-t'-il encore les dessins pour la partie de votre Carte d'Etat-Major, qui embrasse le Mont-Blanc? Il me serait bien précieux pour mes recherches sur les glaciers de pouvoir me procurer une Copie de cette parti.

c

Author.

P.P. SALLANCHES
CHAMONIX

Monsieur

M. le Chevalier Plana
Directeur de l'Observatoire
de &c &c
à Turin